Date: July 25, 2015

Handling Editor
Earth System Dynamics (ESD)

Dear Dr. Anja Rammig,

Submission of revised manuscript esd-2014-82

Please find attached herewith our revised manuscript, entitled "*Inter-annual and seasonal trends of vegetation condition in the Upper Blue Nile (Abay) basin: Dual scale time series analysis*". We have addressed comments made by the reviewers, as detailed below.

We appreciate the considerable time and effort made by the reviewers, whose inputs have substantially improved the manuscript. We would be happy to answer any further questions.

Yours truly

ErmiasTeferi

Addis Ababa University, Ethiopia

Comment	Response	Changes
Abstract: It has improved a lot. P1 L35 and P2L1: What do you mean with more? More than what?	In the revised manuscript we have replaced the word "more" by "mainly"	Page 2 line 3
P4L22 Which country?	Ethiopia	Page 4 line 26
P6 L6 Incorporate into the manuscript why you used monthly data instead of bimonthly data. A future study is not a reason to use a less good product in this study. I hence still think you should change the analysis to use bimonthly data instead of monthly.	We have done the correction in the revised version.	Page 8 line21-23
P6 L13 It sounds like the MODIS products is already filtered for these conditions when downloaded. This is not the case, write down exactly which quality flags you applied for filtering the data set.	We have added explanation about how we used pixel reliability information for quality control of the data	Page 6 line19-24
P6L26 MDOIS should be MODIS.	We have done the correction in the revised version.	Page 7 line 4
P7. Include the information in response to comment 14, that the HANTS is done iteratively, how many iterations, and that it fits to the upper part of the curve.	We have inserted that in the revised version	Page 8 line 2-9
P7L30. Was the HANTS run on bimonthly data? According P6L7 GIMMS was aggreagated before the HANTS analysis. Be consistent and apply the same method for both MODIS and GIMMS. Specify if the aggregation was done before or after HANTS.	The HANTS was run on the bi-monthly data and the aggregation was done after HANTS.	Page 8 line 19
P7L30 Bimonthly not biweekly	We have done the correction in the revised version.	Page 8 line 19
P8L30 Include in this example that it is for GIMMS data that this example is valid.	We have done the correction in the revised version.	Page 9 line 15
P9L14 Describe what all parameters are. Why only amplitude 0 and 1, why do you not described amplitude 2? Same thing for the phase 2, describe them all.	We have limited our explanation to Amplitude0, Amplitude 1 and Phase 1, as Amplitude2 and Phase 2 are difficult to interpret.	Page 8 line 11-19
P9L24. Again indicate that this is only valid for the GIMMS analysis.	We have done the correction in the revised version.	Page 9 line 19
P10 L15 describe why only 5 major seasonal trends	We have done the correction in the revised version.	Page 11 line 7-10

I still do not understand why to do the petitts change point analysis. If the BFAST gives you more information than Pettitts, why do the Pettitt? If you necessarily want to keep both analysis, you should justify the reason in this subsection.	We have omitted the use of Pettitt test	Section 2.6
P12 L15 What does the percentage stands for? 3.8% per year, would generat a 120% increase over the 25 year study period. Should it not be 0.377%?	We have done the correction in the revised version.	Page 13 line 14-21
P12 L20-P12L27 I am not certain I understand this. Please explain somewhere in the manuscript, either here or in the method section, why a positive difference between linear and MK indicates that linear is better than MK. If a positive difference indicate that linear is better than MK for areas with a positive trend, then a negative difference for areas with a negative trend should again indicate that linear is better than MK. But according the manuscript, MK is better for negative areas since the difference is negative. This result does hence not make sense, in case I understood it correctly.	We have removed anything related to this in the revised version	Page 13 line 23-30
P12 L32. Make sure that the figures and tables come in the correct order. As it is now Fig 7 comes after Fig 2.	We have now corrected the order	
P13 L2 This number cannot be correct. It would indicate that NDVI increased with ~0.83 over the 11 years. It should either be 0.0768/11 years, or 0.00768 per year. Same for the other numbers in this section.	We have double checked the analysis and the number is the same.	
The section 3.2.1 was very well written. In the GIMMS section, you state that you found some major types of changes and then you describe these different classes. So, I do not understand why is not section 3.2.2 about MODIS written in the same manner.	We have done that way for MODIS in the revised version	Section 3.2.2

Why do you suddenly start calling it A1, A0 etc. Previously you have written Amplitude 0. Be consistent, and use the same terminology throughout the paper.	We have done the correction in the revised version.	
P15 L6 There is no subplot (a) in Fig 8.	We have done the correction in the revised version.	
P15 L8-9. The dominant classes are never described, they are only included in a legend in Fig. 8. Why do you not do as in subsection 3.2.1 describe the main classes you have found, what they look like, and where they are located?	We have done the correction in the revised version.	Section 3.2.2
Why do you suddenly start talking about ROIs. In the method section ROIs are not included before the change point analysis. I hence suggest to describe the main trend classes that you found in the MODIS analysis. Based on these trends, and the trends you found in GIMMS, you choose some ROIs where you study change point analysis. These ROIs should hence be described in the next section about trend break analysis.	We have used ROI for increasing trend pixels and decreasing trend pixels as samples for trend break analysis.	Section 3.3
Subsection 3.3.1. Not clear what the different ROIs are representing. Describe what all ROIs are representing.	We have used ROI for increasing trend pixels and decreasing trend pixels as samples for trend break analysis.	Section 3.3.1
P16L26 Why is it suddenly classes? According the method section, the trend break analysis was done in ROIs, not for classes?	We used the ROIs to represent the most prevalent classes of trends in seasonality.	
Subsection 3.3 I still do not understand why you did the Pettitts change point analysis. The BFAST analysis seems to generate much more robust results. Looking at Fig 10, I do not see any break in the NDVI time series at 2004 where it was indicated in the Pettitts analysis that a trend break should be seen.	We have used BFAST for all analysis in the revised version.	
P18 L13 Include the info about burning that you gave in the response to comments.	We have included the information in the revised version	Page 22 line 13

P18 L23 No, a significant increase I mean NDVI but without any changes in the other shape parameters do not indicate a lengthening of the growing season. It is very clear in Fig 4a, that the curve is simply moved upwards, the growing season is not longer. You had changed this according to your response to comment #27.	We have done the correction in the revised version.	Page 22 line 30
It has previously been shown, based on GIMMS data, that the greening trends in the Sahel is mainly attributed to an increase in the maximum values, whereas the length of the growing season has not increased (Heumann et al 2005).	We have done the correction in the revised version.	Page 23 line 4
What is myriad? Please then give references	We used "myriad" to mean "several"	
Your main conclusion on P19L2-3 is still correct. Yes the increase in vegetation greenness indicates increased vegetation productivity, but not due to a lengthening of the growing season but due to an increase in greenness.		
P19L9-10 please rephrase. Hard sentence to understand.	We have deleted "to"	Page 23 line 17
P19L14 Could forest plantation be a cause?	Forest plantation could be the cause, but its impact can be detected in neither 8km nor 250m data. Because Eucalyptus planation is carried out mostly at household level on farmers plot, which is smaller than 250mx250m.	
P20 L1 According results and fig 8 it should be 5 vegetation changes?	We have done the correction in the revised version.	Page 24 line 11
P20 L4 I do not agree with this statement. It looks like ROI 5 and ROI 8 had double season in both 1981 and in 2006? Since they have double seasons in both periods it does not indicate a change from shrubland to irrigated double cropping.	We have done the correction in the revised version.	Page 24 line 15
P20 L5 I would not consider a change in NDVI for ROI 3 from an ~average of 0.84 to ~average of 0.83 a significant change indicating degradation. Please use same scale on y-axis to clarify these things in fig 9.	This mainly shows a decrease in vegetation density. So, it is still a valuable information.	

P20 L13-L17 Again, I do not agree with the results of the Pettitts analysis. Are you sure that they are correct. Looking at the time series of NDVI in fig 10, it is clear that there are browning trends, but I cannot see any clear breaks in the point in time where Pettitts change point analysis claim to find breaks in 2004-2006. Again, I would recommend to stick to BFAST.	We have omitted the use of Pettitt test.	
P20L18=> very good discussion about possible reasons for the browning trends.	Thanks for the compliment.	
P22L15 Do you have any explanation for why it is highest in the Belles sub basin?	Maybe due to the current deforestation going on for the purpose of sugar cane planation.	
Tables and figures		
Table 6. When looking at the time series of NDVI in Fig ure 10 I do not see these break points. I would instead trust the BFAST. According the legend there should be negative signs in the table, but there are none.	We have used now only BFAST	
Fig. 2 Clarify which color is 0 in (a), (b), and (c). Why do you show mean and covariance in Fig 7 but not here? Use the same color scale in c) and d), to make it easier for the reader to compare the results.	We have changed the figure in the revised version.	Figure 2
Fig 3 is never referred to, and there is something wrong with the figure: no legend, no latitudes, no figure in subplot(b).	It is referred now in the text. We have correctly displayed now.	Figure 4
Fig. 5 Why is not all ROI presented. 8 subplots is not too many. Explain which ROI describes which class in the figure legend.	We just took samples for increasing and decreasing trend. We felt that all plots are not essential.	
Fig. 6 Which ROI are these different classes? In (b) perhaps include amplitude 0 on a second y-axis.	We have removed that figure and replaced it by Figure 9	Figure 9
Fig. 7 In fig 2 the difference between MK and linear is included, why is this not done in figure 7? The year is missing in the reference (Shukla et al)	We have removed that in the revised version. (Shukla et al)—this reference shouldn't be there. It is an error.	
Maybe combine fig 8 and fig 3? Change ROI from grey to black, they are very hard to see as it is now. Latitudes are missing.	We have increased the visibility of sampled locations in the revised version.	Figure 2 and Figure 3

Fig. 9 Why not using the same scale on the y-axis for all subplots, makes it easier to compare. Why not include the first and last year of the GIMMS data for the ROI in these plots as well. Would further highlight when the changes take place. According the figure legend, the ROIs are based on the amplitude composite image Fig 8, whereas according the method section, they are based on the classes in both GIMMS and MODIS?	When we make the make the same scale, it would be hard to visualize the difference.	
Fig 10 Why did you not present the analysis for all 8 ROIs?	We have now presented the analysis for all ROIs.	