First Reviewer:

We would like to thank the reviewer for his comments and thoughtful suggestions.

Comment 1:

General Comments-Overall Quality of Manuscript The manuscript could be better organized than it is now and be written in a much less confusing fashion. There is so much mention about work not actually done in the manuscript that the manuscript discusses. The authors do not make much effort to look and acknowledge the existence of the latest efforts, studies and publications in the variations of Ethiopian rainfall, global SSTs, and their teleconnections. This manuscript must not be published in its current form. The manuscript needs major revisions and corrections to be suitable for publication.

Reply 1:

The authors have reorganized some sections in the paper including the introduction and Section 3 to make it more clear for the readers to understand. Several new recent publications that are highlighted by the reviewer are included in the modified manuscript.

Comment 2:

Specific Comments- Scientific Questions Identification of nonstationary global and regional SSTs variability and the nonstationary variability of rainfall and streamflow (e.g., Ethiopian rainfall, East Africa rainfall, Runoff of Upper Blue Nile basin) and their teleconnections to local and global SST variations has in recent years been investigated and established by Elsanabary et al. (2013), Mwale et al. (2004 and 2007) and Mwale and Gan (2005). The paper by Elsanabary et al. (2013) clearly showed the nonstationary linkages between SST (including southern Indian Ocean see Figure 12(a)) and Ethiopian rainfall). The authors should acknowledge those discoveries. 1. Mohamed Helmy Elsanabary, Thian Yew Gan and Davison Mwale: Application of wavelet empirical orthogonal function analysis to investigate the nonstationary character of Ethiopian rainfall and its teleconnection to nonstationary global sea surface temperature variations

for 1900–1998 INTERNATIONAL JOURNAL OF CLIMATOLOGY (2013) Published online in Wiley Online Library (wileyonlinelibrary.com) DOI: 10.1002/joc.3802 2. Mwale D, Gan TY. 2005. Wavelet analysis of variability, teleconnectivity and predictability of the September-November east African rainfall. Journal of Applied Meteorology
44: 256–269.

Reply 2:

The highlighted papers are discussed in the modified manuscript.

Comment 3:

Technical Corrections Abstract: Page 4852-Lines 2-5: These lines need to be removed. They are not relevant. The manuscript can start from Line 6. Page 4852-Lines 5: Replace “Here we analyze”, by “This study analyzed”. Remove all references to “we” in
the, manuscript and replace them with “This study” Page 4852-Lines 10: This statement is needs to be revised in light of Elsanabary et al. (2013), Figure 12. Page 4852-Lines 14: Replace “we estimate”, by “This study estimated” Introduction: Page 4853-Lines 15-25 and Lines 25-28: These portions of the manuscript have no relevance to the study. The authors should begin at line 20. . .”The UBN. . .” This manuscript is about Ethiopian rainfall /UBN runoff. References to “East Africa or Eastern Africa rainfall are irrelevant” and should be removed. Page 4856-Line 5: References are too old: There are many studies done after 2003: 1. Diro G, Grimes D, Black E. 2011. Teleconnections between Ethiopian summer rainfall and sea surface temperature: part I-observation and Modeling. Climate Dynamics 37: 103–119. 2. Beyene EG, Meissner B. 2010. Spatio-temporal analyses of correlation between NOAA satellite RFE and weather stations’ rainfall record in Ethiopia. International Journal of Applied Earth Observation and Geoinformation 12: S69–S75 3. Gissila T, Black E, Grimes DIF, Slingo JM. 2004. Seasonal forecasting of the Ethiopian Summer rains. International Journal of climatology 24: 1345–1358.

Reply 3:

The suggested modifications and publications are included in the modified manuscript.

Comment 4:

Page 4857-Lines 20: Where is the algorithm developed by this paper? Where is it explained in detail?

Reply 4:

The algorithm is discussed in section 4 and Figure 6 summarizes the forecast steps.

Comment 5:

Page 4858-Lines 9-21 This manuscript is supposed to be discussing the use of observed SST data and streamflow. All references to dynamic models and their data should be removed. Why would the authors go on and on talking about dynamic models and their SST data when they acknowledge earlier that its beyond the scope of the manuscript?
Page 4858-Lines 22 How does the method change to dynamic when the manuscript is supposed to be using observed data?
Page 4862-Lines 16-18 This manuscript should keep to talking about what the study accomplished not what the authors wish.

Reply 5:

The authors wanted to describe a complete framework that is able to predict the Nile flow with several months ahead. The dynamical models are the only tools that can be provide global SSTs forecast several months ahead. However, these models are still under development and their accuracies are still not very high. This prevents the authors from using them at this stage. The authors used the observed SSTs as a replacement of the forecasted SSTs from the dynamical models to test the accuracy of the suggested forecast method to predict the Nile flow. Such test describes an upper limit of the skill of the proposed algorithm.

Second Reviewer:

We would like to thank the reviewer for his comments and thoughtful suggestions.

Comment 1:

I have two problems with this manuscript. Firstly, I am not at all sure what, if any, of the findings can be considered to be novel. One of the main findings is that the SOI index is useful in describing the flow in the Nile, but this appears to be well-known from work the authors have already published in Siam et al (2014) – page 4855, line 27-28. As such the correlations derived in his study can hardly be considered surprising, but the existence of these connections reported as one of the main findings (first bullet point in the conclusion).

Reply 1:

The published work of the authors in Siam et al (2014) is only discussing the physical mechanism that connect the South Indian Ocean and ENSO to the Nile flow and did not discuss how these connections can be used in forecasting the Nile flow or the different modes of natural variability in the flow of Nile river. The authors summarize this mechanism in the introduction. In this study, we build on this work and describe the different modes of natural variability in the flow of Nile river and how we can combine the SOI index with ENSO to accurately forecast the Nile flow. This study also has an advantage compared to the others similar studies as it is based on physically explained correlations between the Nile flow and SSTs indices. Most of similar studies are based on random correlation found between the Nile flow and global SSTs, which increases the confidence in the suggested forecast method.

Comment 2:

Secondly, in Section 3 the connection between flow and SOI & ENSO is established through a simple linear regression (Table 1). I am not entirely sure I understand how the 'neutral, and 'non-neutral' classes are derived, but as far as I can work out the conditional probabilities reported in Section 4 are based on slightly more refined subsets of SOI and ENSO and therefore offer no real insight that has not already been reported in section 3, or could not easily have been derived from the regression analysis. In any case, it was already well-known that these correlations existed (see comment above). I like the forecasting index in Eqs. (3) and (4), but this also appears to have been developed in a previous study (Wang and Eltahir, 1999).

Reply 2:

The authors modified this part in the manuscript to make it more clear for the readers to understand the difference between neutral and non-neutral years and the regressions are made. In this study, we suggest a framework that is able to predict the Nile flow with several months ahead. The authors focuses in Section 3 in describing the different modes of Natural variability in the flow of Nile river, while Section 4 describe how these modes can be used in the forecast algorithm.

Comment 3:

I found the introduction difficult to read. I think the authors should provide a more structured discussion of previous findings and listing of the main drivers. For example, it is difficult to understand if the rainfall over East Africa is important or not (page 4853, line 15-28). In the last paragraph of the introduction there are numerous climatological phenomena introduced, and I would have liked to see a more order discussion, possibly aided by a figure or a table, to enable a better assessment of the novelty and validity of the subsequent analysis.

Reply 3:

The authors modified this part in the manuscript to make it more clear for the readers to understand.

Comment 4:

Page 4856, line 18: This reference to Table 1 comes before any discussion of the analysis and results presented in the Table.

Reply 4:

The reference in this line is removed and placed in the discussion of this part.

Comment 5:

Table 1: When using both ENSO and SOI the R2 increases, but is this not a normal consequence of using two rather than one explanatory variable? Maybe this is worth a comment?

Relpy 5:

The authors describe how the addition of the SIO index to ENSO improves the explained variability in the flow of Nile river. This is mainly because the SIO index can explain some additional variability in the flow of Nile river that is independent from ENSO. For example, when the SSTs over the North and Middle of the Indian Ocean were used with ENSO index, they did not explain any new variability in the flow of Nile river as the SIO index did. The SSTs over the North and Middle Indian Ocean are dependent on ENSO, hence, their use in presence of ENSO did not increase the explained variability in the flow of Nile river. This discussion is added in the modified manuscript. A new figure is added to show the improvement due to adding SIO index in the Forecast (Figure 4 in the modified manuscript).

Comment 6:

Page 4856: I think more details of how the ‘neutral’ and ‘non-neutral’ years were classified –perhaps add a figure with a time series of SST and to illustrate the variability?

Reply 6:

The authors modified this part in the manuscript to make it more clear for the readers to understand.

Comment 7:

Page 4859, line 8: there is no Winkler (1972) in the references.

Reply 7:

The reference is corrected in the modified manuscript.