

Interactive comment on “Explaining and forecasting interannual variability in the flow of the Nile River” by M. S. Siam and E. A. B. Eltahir

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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 This manuscript must not be published in its current form.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.

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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.

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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 ob-
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served 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.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/11/C4254/2014/hessd-11-C4254-2014-supplement.pdf>

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 4851, 2014.