Clim. Past Discuss.,
doi:10.5194/cp-2015-194-RC1, 2016

Climate of the Past Discussions

Interactive comment on "Comparison of simulated and reconstructed variations in East African hydroclimate over the last millennium" *by* F. Klein et al.

Anonymous Referee #1

Received and published: 11 March 2016

The manuscript presents a comparative analysis of proxy records representative of hydroclimate in Eastern Africa and corresponding time series from climate simulations over the past millennium. After discussing the caveats due model spatial resolution and spatial homogeneity of precipitation this region, that authors reach the main conclusion that most of the hydroclimate variability in this region is probably caused by internal process and that the influence of external forcing seems to be very limited, in agreement with other studies that have pointed out the importance of internal variability in hydroclimate other parts of the world. Another important conclusion is that the different models do not agree in simulating the links between hydroclimate and sea-surface-temperatures. I think the research question is important and opens up fur-

ther questions, as for instance the reasons why models diverge when simulating the SST-hydroclimate link which also leads to the question of the origin of hydroclimate variability itself and its connections to global patterns of climate variations like ENSO or the Indian Ocean dipole. My general impression of the manuscript is quite positive. The manuscript is rather long and, although at some stages the study falls short of reaching robust conclusions, I think it is a worthwhile contribution and opens up some lines of research for further studies. I liked the amount of manuscript space devoted to check the spatial representativity of the hydroclimate records, the skill of the models in simulating the two different precipitation annual cycles and the teleconnections to the large-scale SSTs, although I have a comment on this last point.

I have some comments on the manuscript that the authors may want to consider. Only two of them are general enough to possibly require some major changes in the manuscript, the rest being more more specific.

-I would like to start, however, underlying that the submitted version does not appear to have been thoroughly revised by the authors. Something seems to have gone awry regarding the blank spaces to separate words, and may words throughout the manuscript appear juxtaposed , at least in the pdf copy I downloaded. This has made the reading quite uncomfortable. This impression is confirmed by the acknowledgements to Flavio (?). I believe it is appropriate to acknowledge him by his full name.

-My main concern is the claim that precipitation, relative to evaporation, is the main factor driving hydroclimate variability. The authors compare the standard deviation of precipitation and evaporation in the model output and reach the conclusion that the former is much larger, with a few exceptions in the Challa/Naivasha region. However, this calculation is done at interannual timescales, as far as I can judge comparing the much larger magnitude of the standard deviations shown in Figure 6 than those shown in Figure 8, which are explicitly calculated at centennial timescales. If this is correct, I think this conclusion could be premature, since at longer timescales the variability of temperature would likely grow relative to the variability of precipitation, and thus also

the role of evaporation could become more important. I think this should be checked because the authors base some of the further analysis on this conclusion, and because it is a quite relevant conclusion on its own right.

-Another point is related to the teleconnections between precipitation and sea-surface-temperature described in section 3.3. This section directly assumes that the SST is a direct driver of precipitation, but the text does not contain a justification for this assumption. Could it be that both SSTs and precipitation are driven by the atmospheric circulation? In this region, both may be coupled being part of some coupled mode of variability, but it is also possible that the atmosphere is driving both. This possibility is related to the main conclusion of the paper that the influence of external forcing is negligible, as the atmosphere circulation would be arguably less responsive to forcing than the SST.

Particular points:

-The 1000-year time series representing hydroclimate variation in the Lake Challa region in Tierney et al. (2013) is the first principal component of composite variation in three moisture-balance proxies, namely a presumed indicator of catchment.

It would be useful to quote the variance explained by this leading PC. Is it clearly over 30%, which would be the expected value if the three series were uncorrelated ?

-pattern during an El Niase of ENSO

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-the series has been linearly standardized so that the maximum of theabsolute values equals 1.

This standardization is not really robust, as it depends on one single value: the maximum element in the series. The amplitude of the standardized series may therefore depend on an outlier.

-I really had to wrestle to understand Figure 9. First, I could not see the individual simulations of the CESM ensemble in panel upper row centre, apparently drawn with different shades of grey according to their distance to the median. I do not think it is necessary to show the distance to the median (what is the reason ?), and it quite messes up the figure. What would be the distance criterion anyway ? Second, I did not understand the blue shading. It apparently shows the 2x standard deviation derived boundaries from a control run, added to the median simulation (?). But it seems that the blue shading is not simply the median line with (constant) 2xsigma boundaries added. The blue-shaded area has a time evolution that is different to that of the median simulation. Third, if the blue-shading indicates the standard deviation from control simulations, it is much smaller that the standard deviation from the forced simulation , so why is the line indicating the latter dashed? My impression is that this caption is not quite right. Maybe the blue shading indicates the within-ensemble standard deviation from the CESM ensemble (and so it is time-evolving as well) and not the standard deviation from a control run (constant). I think I see the point that the authors are trying to make in this panel. Perhaps the authors may want to consider just showing one or two simulations from the CESM ensemble if what they wish is to convey the amplitude of variations as compared to the other models. Showing the median is misleading (compared to other models), and showing the within-ensemble standard deviation (if this is what the blue-shading indicates) does not alleviate the problem.

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