Responses to Anonymous Referee #2

For clarity we repeat the reviewer comments in blue italic font and the replies are in black.

In their manuscript on "Interannual variability of isotopic composition in water vapor over West Africa and its relation to ENSO", Okazaki et al. validate their model results with some observations and find an interesting relationship between vapor isotopes and ENSO for paleoclimate reconstruction. This work is particular of interest for isotope and paleoclimate communities because paleoclimate proxies have higher than annual resolution, recently. It is, therefore, necessary to understand how climate dynamics with shorter timescales, particularly the seasonal timescale like this work.

The manuscript is well written, referenced and clear to the reviewer. I recommend a publication of the manuscript. Some minor issues are listed below.

Thank you very much.

1. *The authors presented Fig. 1 and Fig. 2 to show validity of their model results compared to the remotely sensed data. Then, I would like them to show concentrations of water vapor itself on top of the isotopic composition of water vapor. By doing that, potential readers convince the model results better.*

   We added the figures of the concentrations of water vapor in Fig. 1 and Fig. 2. We compared the simulated precipitable water with reanalysis data (JRA25). Though the model has moist bias over the Sahara, the absolute value and seasonal variability is well captured. Thank you.

2. *p24451, line 12-14, the authors connect their comparison results with global circulation, monsoon flow, and convective activity. Among them, convective activity can't be explained in this model results. Rephrase the sentence or explain more.*

   We meant that the seasonal pattern of vapor isotope ratio driven by convective activity is well simulated. According to Tremoy et al. (2011), the observed intra-seasonal variability of $\delta^{18}O_v$ at a site in West Africa is associated with convective activity and our simulation result was fairly comparable with their observation. Thus our comparison result indirectly supports that our simulation captures the effect of convective activity. We will refine the sentence to make this point clearer in the revised manuscript.

3. *Please specify the definition of d18O and dD. In addition, need to be addressed why the authors use either dD or d18O in each case.*

   Thank you for your comment. We have included the definition of d18O and dD in the revised version of the manuscript. Since some observations measured only dD or d18O, we had to use both dD and d18O to directly compare the model and the observations.

4. *In either introduction or conclusion and perspective, some paleoclimate studies over the Western Africa need to be addressed to introduce this study to potential readers.*

   We added the review of paleoclimate studies over West Africa, such as Azzoug et al. (2012) and Schöngart et al. (2006), in the introduction section in the revised version of the manuscript. Please note that we referred a paleoclimate study by Shanahan et al. (2011) at the conclusion section in the first version of the manuscript.