Replies to Interactive comment on "Effects of changes in moisture source and the upstream rainout on stable isotopes in summer precipitation – a case study in Nanjing, East China" by Y. Tang et al.

Anonymous Referee #1

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Note: The reviewer's comments are in black, our replies in blue, and the changes in the text marked in red.

Summary: In this paper, the authors document the isotopic variability of monsoonal precipitation at Nanjing, China, and explore the ways in which atmospheric circulation, moisture source and upstream rainout affect this variability. One motivation for this investigation is that oxygen isotopes, particularly those from Chinese speleothems, are used to infer changes in the amount of Asian monsoon rainfall based on the "amount affect." Recent work, including that reviewed here, however, suggests that the amount affect is weak or non-existent in many monsoon regions and thus alternative reasons for isotopic variation in precipitation need to be explored. Here, the authors suggest that the location of convection and changes in moisture source regions are important factors that impart significant isotopic variability on $\delta^{18}O_{precip}$. In particular, the position of the ITCZ and associated convective maxima is considered. Overall, the paper as presented is interesting and high quality. It is well written and I enjoyed reading about the different influences on monsoonal precipitation and its isotopic composition. The methodology appears to be

sound and the results are both interesting and significant for the interpretation of paleoclimate records. There are two major comments that I have, however, about the paper as it stands. Below, major comments are addressed first and are followed by more specific comments.

Decision: My recommendation to reconsider after major revisions is based on the assessment that a major motivation of the work is to help improve the interpretation of paleoclimate oxygen isotope records, namely speleothems. In order to do this, monsoonal and non-monsoonal precipitation/isotope processes need to be considered. Here, only the monsoonal season is considered. If reframed as an investigation of those factors that affect the isotopic composition of monsoonal precipitation only, then the paper could be accepted with minor revisions.

Major Comments: 1. Despite a lengthy introduction that recognizes changes in the proportion of monsoon and non-monsoon precipitation is important in influencing the average isotopic composition of precipitation, the study presented here only addresses those factors that influence monsoonal precipitation. Since the authors acknowledge in their introduction that one reason speleothem $\delta^{18}O$ interpretations in terms of monsoonal amount affect are potentially flawed is that precipitation at cave sites is not exclusively monsoonal, a more complete investigation would consider all the factors that contribute to the average annual isotopic composition of precipitation in addition to those factors that influence the isotopic composition of monsoonal precipitation.

2. Related to the above comment, a discussion that addresses the most important

factors that control both the average monthly and annual isotopic composition of precipitation would benefit the paper. Indeed, by discussing the controversy surrounding the interpretation of Chinese speleothems in the introduction, the authors set up the need to address factors that control the annual average isotopic composition of precipitation, which is preserved in speleothem $\delta^{18}O$ records. If the goal of the paper is not to help address the interpretation of speleothems, but instead to provide insight in the factors that contribute to monsoon season precipitation, then the introduction should be modified to reflect this. Otherwise, a more complete discussion on the factors that control the monthly and/or annual average isotopic composition of precipitation should be undertaken.

We agree that the non-monsoonal precipitation/isotope processes need to be investigated because the annual average isotopic composition of precipitation, which is preserved in speleothem oxygen isotopic records ($\delta^{18}O$), is controlled by both the summer and winter monsoons. However, we focused our analysis on summer because the "amount effect" is most prominent with summer precipitation in the monsoon region because of the relatively high intensity of summer precipitation events, often involving strong convective processes. In addition, precipitation concentrates in summer in monsoon region. According to long term monthly means of Nanjing precipitation for the years 1981-2010 from the China Meteorological Data Sharing Service System (http://cdc.nmic.cn/home.do), summer precipitation (June-September) accounts for 54.8% of its annual precipitation. Therefore, in order to determine whether the "amount effect" is the predominant mechanism for the isotopic variations

of precipitation in the Asian monsoon region, it is important to examine summer precipitation in details. Such results will be of great value to paleohydroclimate reconstructions using speleothem isotopic records as they are often interpreted as a proxy for monsoon intensity as indicated by monsoon season (summer) precipitation amount. The isotopic variations of precipitation in winter/non-monsoonal season (October-May) are controlled by different processes, and its contribution to the annual precipitation-weighted mean isotopic composition has not been fully assessed when interpreting isotopic records in speleothems in the Asian monsoon regions. These are important issues that will be addressed in a follow-up study, hence are not discussed in this paper.

Specific Comments

Line 84: Vuille et al. (2005) is another reference that could be cited as showing that convection in core monsoon moisture source regions and along moisture source pathyways in Asia contributes to the isotopic composition of precipitation.

The reference (Vuille, M., Werner, M., Bradley, R. S., and Keimig, F.: Stable isotopes in precipitation in the Asian monsoon region, J. Geophys. Res., 110, D23108, doi: 10.1029/2005JD006022, 2005.) was added in the revision.

Lines 95 – 102: Why not also address winter precipitation and isotopic variability since the data are available? It seems logical to do this since the set up for the paper was with respect to the interpretation of isotopic records that reflect annual averages.

See above.

Line 126: The authors here refer to BOB as the Bay of Bombay when in the rest of the paper it appears that the Bay of Bengal is being referred to as BOB (Line 156). Please clarify. Also, is it necessary to abbreviate to BOB in the first place. It's not clear that this and some other abbreviations are necessary. Eliminating some would help the manuscript's readability.

We made a mistake in referring to BOB as the Bay of Bombay. Throughout the paper the BOB refers to the Bay of Bengal. In the revised paper, we deleted the abbreviations of BOB (Bay of Bengal) and SCS (South China Sea), and used their full names instead.

Line 220 – 221: Add a reference for this sentence.

The reference (Dansgarrd, W.: Stable isotopes in precipitation, Tellus, 16, 436-468, 1964.) was added here.

Line 224: Clarify "local water." I presume local surface waters like lakes and streams are being referred to?

Yes, "local water" refers to local surface waters like lakes and streams, which was clarified in the revision.

Line 229: Delete "Results are shown in Fig. 4." and add (Fig. 4) at the end of the

previous sentence.

Changes were made accordingly in the revision.

Line 232: Change “evaporation ratio” to evaporation/precipitation ratio.

Changes were made accordingly in the revision.

Lines 233 – 235: Satements in these sentences are interpretation and should be moved to the discussion. Also, it might be worth adding that the amount affect can still play an important role, particularly during times in the past when precipitation was greatly increase or decreased under different climatic boundary conditions.

These sentences were moved to the discussion, and a brief discussion about the amount effect was added in the text as suggested by the reviewer.

Line 248: A brief discussion framing why the ITCZ is being considered specifically and its role in monsoonal climatology would benefit the introduction to the discussion.

A brief introduction of ITCZ and its role in monsoonal climatology were added here. See more details in the revised paper.

Line 257: I don’t believe that the method used to calculate the vertically integrated mean water vapor transport was described in the methods.

The method of calculating the vertically integrated water vapor was described in the

revision.

Line 259 – 264: It is difficult to distinguish the terrestrial boundary in this figure. White lines on top of the contoured meteorological data could help visually. Also rows should be titled with the year each represents and columns should be labeled with the stages that each represents. This will help guide the reader. A more prominent marking of the study site would also be helpful.

Changes of this figure (Fig. 5) were made according to the reviewer's suggestions.

Line 283: Use of BOB and SCS doesn't help the flow of the sentence. I might suggest not using these acronyms.

The abbreviations BOB (Bay of Bengal) and SCS (South China Sea) were deleted and their full names were used instead.

Line 282 – 284: Suggested change: "...convection in the Bay of Bengal and South China Sea (Fig. 5a, f, k), and the delivery of moisture from both regions (Fig. 6a, f, k).

Changed as suggested.

Line 290: The decreased precipitation referred to here is difficult to see as significant in Fig. 6. Perhaps quantify the

Yes, the decrease in precipitation in the South China Sea (Fig. 5g) is difficult to see from the water vapor transport in the South China (Fig. 6g), but the significant

decrease of water vapor transport in the low-latitude western Pacific Ocean is clear (Fig. 6g).

Line 298: It is also difficult to see the ITCZ intensity change described here in Fig. 5.

The decrease in ITCZ intensity in the Bay of Bengal described here was weak for stage 3 of 2012 (Fig. 5c), but was very clear for stage 3 of 2013 (Fig. 5h). Furthermore, the described increase of the ITCZ intensity in the South China Sea and the low-latitude western Pacific Ocean was also clear (Fig. 5c, h).

Line 270 – 275: Like Fig. 5, but more so, it is very difficult to distinguish where the terrestrial boundaries are located and as a result it is hard for the reader to easily follow the discussion that refers to this this figure. White lines for terrestrial boundaries would help. Headers for rows and columns, like suggested for Fig. 5, would be good to add.

Changes of this figure (Fig. 6) were made according to the reviewer's suggestions.

Lines 335 – 345: Why were the time periods preceding each stage chosen? Is it simply that these periods showed the highest statistical correlations? It would be good to clarify this.

Yes, it is simply that these periods showed the highest statistical correlations. This was clarified in the revision.

Line 378: $\delta^{18}O$ remains enriched despite elevated precipitation?

Yes, the isotopic composition of precipitation in late September (stage 5) remains enriched due to the retreat of the summer monsoon, despite the precipitation amount during this period is a bit higher (Fig. 3).