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

Editor Initial Decision: Reconsider after major revisions (22 Jul 2015) by Lixin Wang

Comments to the Author:

Reviewers asked about the non-monsoon rainfall isotope aspect because they contribute to ~50% of the annual rainfall. The authors argued that ""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." This is not sufficient to me. Is the "amount effect" a major factor in paleoclimate reconstruction? This needs to be elaborated.

We agree with the comments of the reviewers and the editor. The isotopic variations in non-monsoonal precipitation were investigated in this paper. As a result, the title of the paper was changed a bit as "Effects of changes in moisture source and the upstream rainout on stable isotopes in precipitation – a case study in Nanjing, East China". See more details in the revision.

***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

Received and published: 18 May 2015

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.

We agree with the reviewer's comments. The isotopic variations in non-monsoonal precipitation were investigated in this paper. As a result, the title of the paper was changed a bit as “Effects of changes in moisture source and the upstream rainout on stable isotopes in precipitation – a case study in Nanjing, East China”. See more details in the revision.

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.

A complete discussion on the factors that control the isotopic variations in summer and non-monsoonal precipitation were presented in the revision.

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 path ways 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.

The isotopic variations in winter precipitation were addressed in the revision.

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. 8 in the revision) 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. 8g in the revision) is difficult to see from the water vapor transport in the South China (Fig. 9g in the revision), but the significant decrease of water vapor transport in the low-latitude western Pacific Ocean is clear (Fig. 9g in the revision).

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. 8c in the revision), but was very clear for stage 3 of 2013 (Fig. 8h in the revision). Furthermore, the described increase of the ITCZ intensity in the South China Sea and the low-latitude western Pacific Ocean was also clear (Fig. 8c, h in the revision).

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. 9 in the revision) 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. 5 in the revision).

***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 #2

Received and published: 11 June 2015

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

Tang et al. examined the isotopic variations in summer precipitation in Nanjing, East China and aimed to attribute those variations to the effects of atmospheric circulation, changes in moisture source, and the upstream rainout by exploring HYSPLIT model with NCEP reanalysis and the OLR (outgoing longwave radiation) composition analysis. This study, including the literatures reviewed in this manuscript, questioned the isotopic "amount effect" for those oxygen isotopes from Chinese speleothems to inferring changes in the amount of Asian monsoon rainfall. In general, it is an important study, and the manuscript is generally well written. I recommend publication but further improvements in several aspects are needed.

There are two major comments that I have:

1) This paper concluded that changes in moisture source location and upstream rainout effect which should be taken into account when interpreting the stable isotopic composition of speleothems in the Asian monsoon region. This is the main

contribution of this research. However, only variations of summer precipitation in monsoon were analyzed. The author stated that the proportion of summer monsoon precipitation (June–September) at Nanjing accounts for 54.8 percentage of its annual precipitation. It is hard to believe the annual variations of precipitation also have same rules. Besides, it is a pity that d-excess and deltaD results are missing. The d-excess is very useful tools to detect the moisture source. For example Xie et al., (2011).

We agree with the reviewer's comments. The isotopic variations in non-monsoonal precipitation were investigated in this paper. As a result, the title of the paper was changed a bit as "Effects of changes in moisture source and the upstream rainout on stable isotopes in precipitation – a case study in Nanjing, East China". See more details in the revision.

We agree with the reviewer that the d-excess is a very useful tools to detect the moisture source. Our d-excess data exhibits significant seasonal variations, with low values in summer monsoon season (June-September) and high values in winter/non-monsoonal season (October-May) (Fig. 4 in the revision), reflecting shifts in the atmospheric circulation between the summer and winter monsoon. In general, the d-excess in precipitation is controlled by both the relative humidity and sea surface temperature (SST) of the moisture source region. However, the variations of relative humidity and SST in the moisture source regions during summer for this study were not significant (Fig. S1 and Fig. S2). Thus, we didn't discuss the d-excess in precipitation in this paper.

2) The conclusions mainly result from OLR and water vapor transport data analysis which make progress comparing with previous studies. Therefore, that information needs to be emphases in results section clearly and the whole paper needs to be reorganized accordingly. In discussion part, there should have included possible uncertainty analysis for the current conclusion. Such as, How about NCEP reanalysis and the OLR composition analysis? Are those methods robust? How about water vapor from local evapotranspiration? Is it negligible? How about the impact of water vapor- precipitation isotopic exchange?

We didn't include the results of the OLR and vapor transport in the results section because we thought it was better to focus on the results of precipitation stable isotopes in the results section.

We agree with the reviewer that possible uncertainty analysis for the current conclusion should be included in the discussion part. The method of NCEP reanalysis and the OLR composition analysis was robust because the isotopic variations of precipitation in summers of 2012-2014 could be well explained by the composition analysis.

The local evapotranspiration is likely to be considerable in the Asian monsoon region because of high vegetation cover under the humid monsoon climate. However how the local evapotranspiration affects the summer precipitation $\delta^{18}O$ was unclear and required further study. We added a brief discussion about the evapotranspiration in the discussion section. In addition, it is possible that the amount affect could still play an important role during some periods in the past as suggested by another anonymous reviewer. We also added the discussion about the possible role of the amount effect in the revision.

We have not examined the impact of water vapor-precipitation isotopic exchange on the isotopic composition of precipitation over the monsoon region. Additional observations are needed to assess this impact.

Minor comments:

1) P3923, L6 : The author cite the (Gu and Zhang, 2002), but it is not find in reference list.

The reference (Gu, G. J., and Zhang, C. D.: Cloud components of the Intertropical Convergence Zone, J. Geophys. Res., 107(D21), 4565, doi:10.1029/2002JD002089, 2002) was added to the reference list.

2) P3923, L12: The daily OLR data was a very important indicator for interpreting variations of isotope composition in precipitation. Thus, there should be explanation for data source and details.

A more detailed description (including data source and other details) of the OLR Data was added here.

3) P3926, L8-10: Was any calibration on the water isotope measurement conducted? Two standards or three standards?

Three internal water standards were used for calibration. Clarification about this was added accordingly in the revision.

4) P3931, L10: Please show the evidence more.

In Fig. 8c, h and Fig. 9c, h in the revision, one could clearly see the shift of ITCZ location northward and eastward and the shorter distance of water vapor transport for stage 3 of 2012 and 2013. The corresponding figures were cited here in the revision.

5) P3939 for Figure 2: Is it possible to show the backward trajectory result for each stage as showed in Figure 5 and 6?

The backward trajectories for each stage are displayed in Fig. S3. In general, most results of the backward trajectory were consistent with our conclusions. For example, the short distance of water vapor transport during stage 3 (corresponding to the shift of ITCZ location northward and eastward) (Fig. S3c, h, m), the distal water vapor transport from the Bay of Bengal during stage 4 (Fig. S3d, i, n), and the water vapor transport from the northern inland areas and the adjacent seas in the northeast during stage 5 (Fig. S3e, j, o) could be clearly identified. However, there were some discrepancies between the result of back trajectory and our result during stage 1. Our result suggests that water vapor transport during stage 1 was from both the Bay of Bengal and the South China Sea, but the trajectory indicates little or no water vapor transport from the Bay of Bengal (Fig. 3a, f, k) during stage 1. The back trajectories of air masses at a location identified using the HYSPLIT model could be sensitive to the choice in height of moisture transport and back time, which may explain the difference between the results of back trajectories (Fig. 3S) and our results. We didn't add the results of the back trajectory in the revision due to the potential uncertainty of the HYSPLIT model.

6) P3944, Figure 7: How to acquire the spatial distribution of daily-18O in precipitation?

Correlation fields between the preceding (prior to n days) daily NCEP OLR and the daily $\delta^{18}O$ in precipitation at a single location in Nanjing were calculated and the strongest correlation fields were displayed in Figure 10 in the revision.

Supplementary Figures

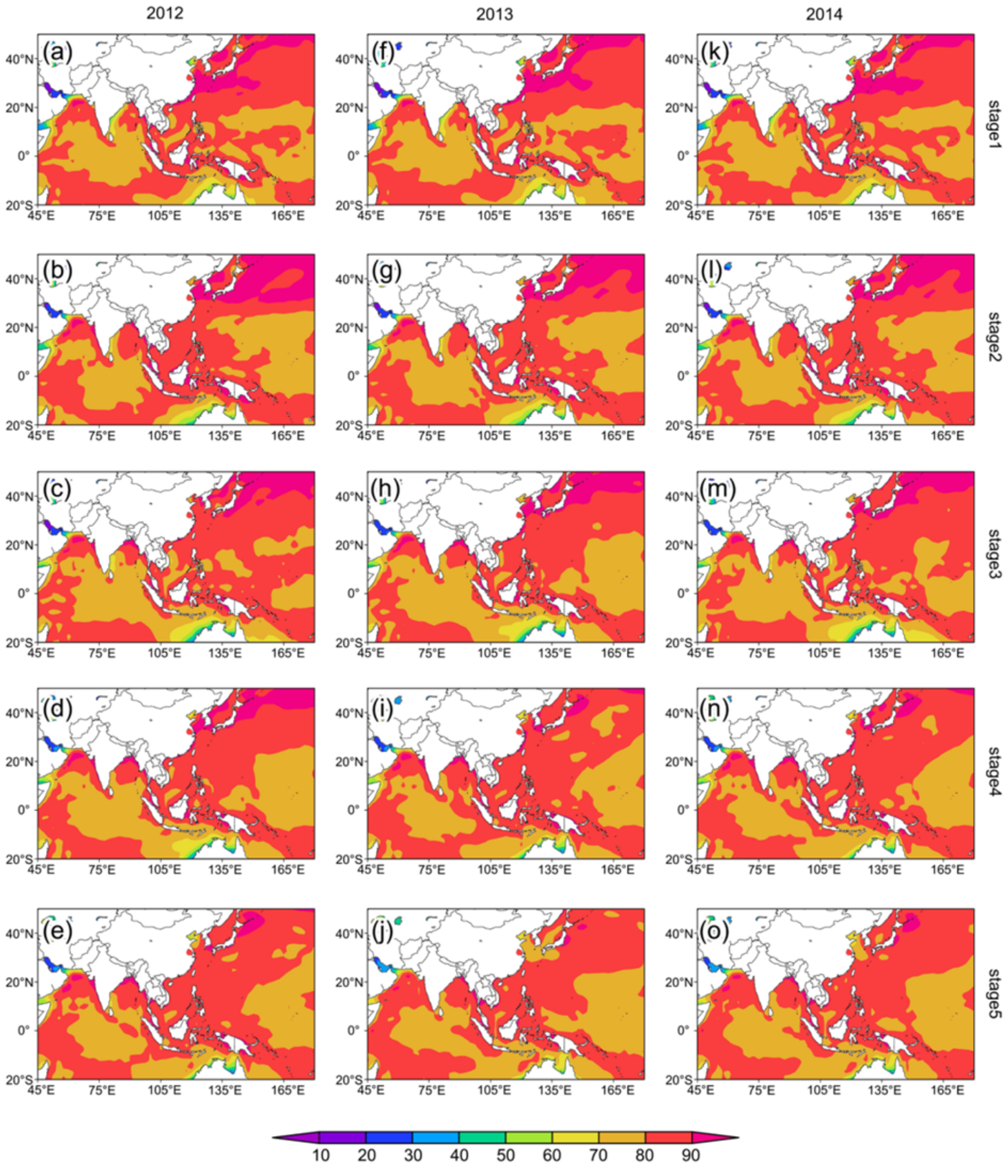

Fig. S1. Similar to Fig. 8 or Fig. 9 in the revision, but for the relative humidity (%) at surface in the moisture source regions.

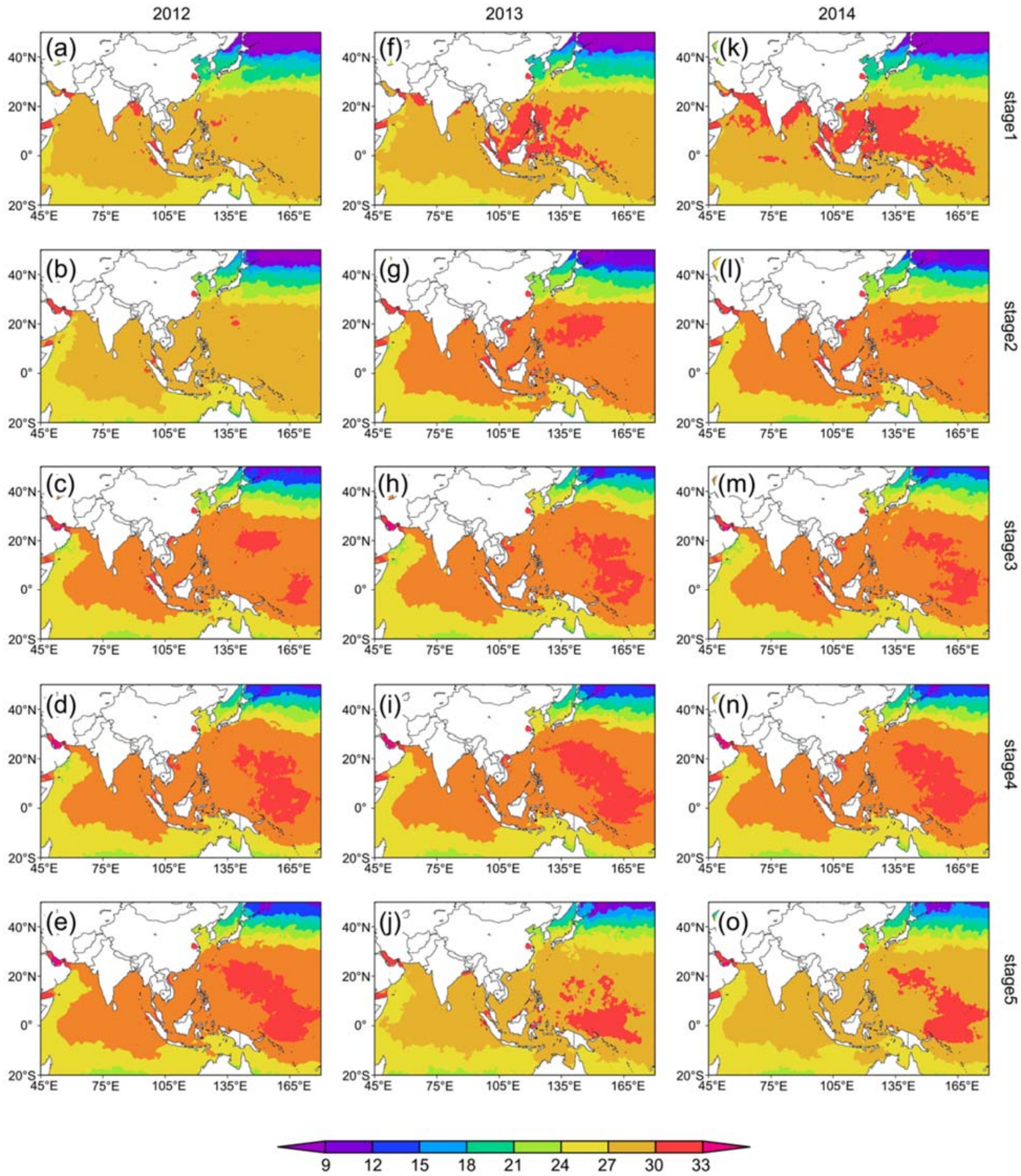

Fig. S2. Similar to Fig. 8 or Fig. 9 in the revision, but for the sea surface temperature (℃) in the moisture source regions.

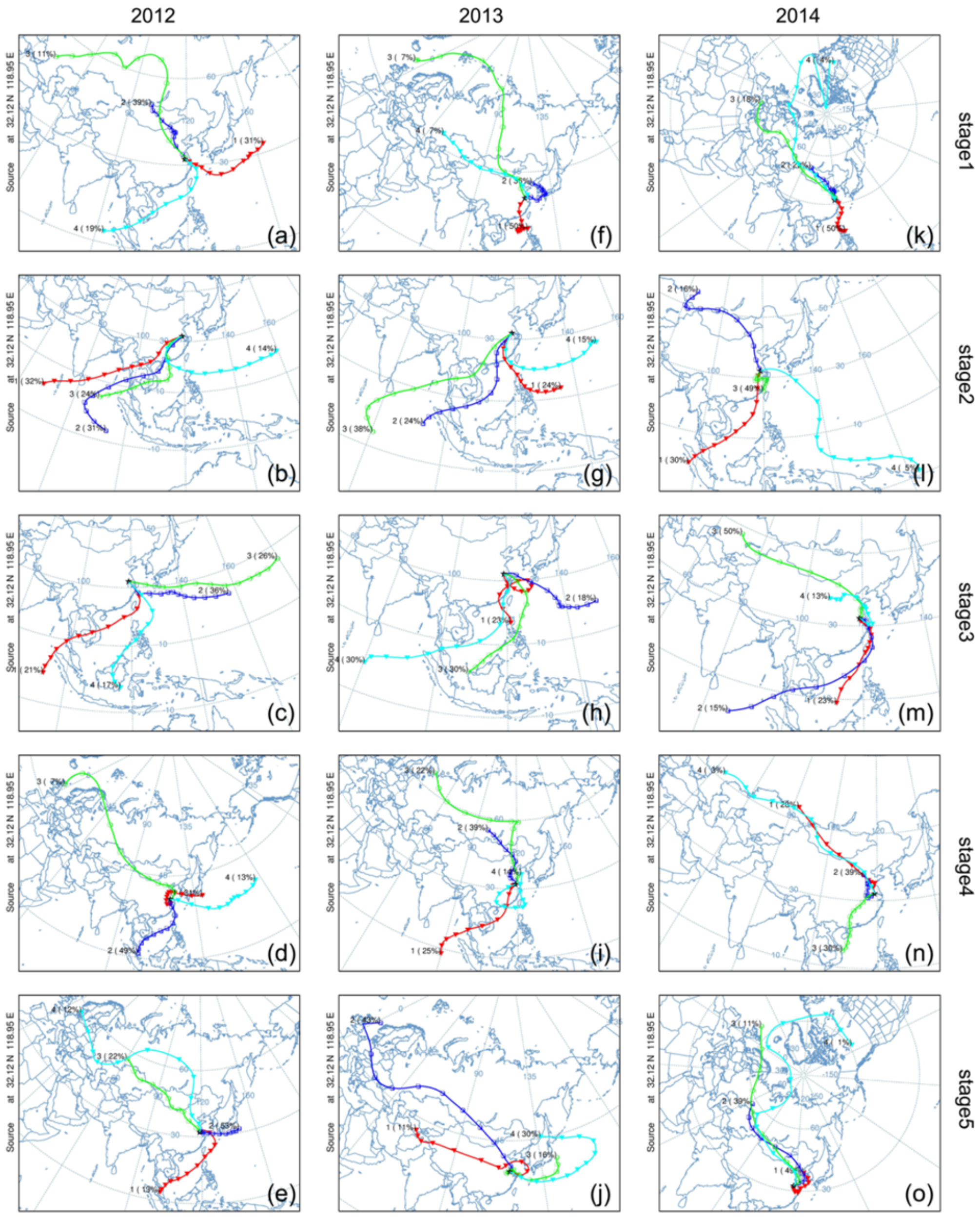

Fig. S3: Similar to Fig. 2b or Fig. 3b in the revision, with backward trajectory results shown for each stage as in Figures 8 and 9 in the revision.