We would like to thank the editor and the reviewer for their time and valuable remarks. Below is our point-by-point response to all the comments made by the reviewer.

Nan S., Tan M. and Zhao P.

Anonymous Referee #2

In this manuscript, Nan et al., present an argument that: 1) there has been an increase in precipitation/stalagmite $\delta^{18}O$ in 1975-1995 relative to the previous two decades and 2) that this change in the isotopic ratio of the precipitation is causally related to a change in atmospheric circulation patterns, which influenced the source regions of the monsoonal moisture. As the authors suggest, there has been a lot of controversy surrounding the interpretation of Chinese stalagmites and it remains a topic ripe for discussion. Although a number of papers including Pausata et al., 2011 (referenced) and Lewis et al., (2010) (Water vapor source impacts...) have dealt with this topic on millennial timescales, the circulation effects have not been well addressed on interannual/decadal timescales over the observational period. To this extent, the paper has some novel analyses and some discussion that is potentially valuable for the interpretation of these controversial stalagmite records. However, the arguments in the paper were ultimately unsubstantiated and do not convince me that, 1) there was clearly an isotopic shift in 1976 and 2) that this shift was necessarily tied to circulation. Part of the failings of the paper may be tied to the writing, which was very unclear and so saturated with acronyms that some potentially good arguments could have just been obscured. A second major failing of the paper is that it took a lot of conjecture from a previous paper (Tan 2013) and presented these hypotheses as though they were well-accepted truths. Granted the other paper is published and therefore can (and should) be referenced but many of the ideas in that paper need more support before they can be used to build new ideas from.

Response: We thank the reviewer for his/her valuable comments that greatly helped us to improve the quality of the manuscript. We principally agreed these comments and have largely modified those which didn't convince the reviewer. Instead of "isotopic shifts in 1976", we reword as "most stalagmite $\delta^{18}O$ series show statistically significant trends of increasing for over the second half of the 20th century, and the increasing trends could be attributed to circulation". Meanwhile, we have realized that our poor English has made the arguments of the paper obscured, so it needed to be checked and modified by a native speaker colleague before the revised draft uploaded. And we also agree with "The ideas in a paper need more support before they can be used to build new ideas from", therefore all the overstatements have been removed in our revised manuscript.

If the Nan et al., manuscript were a simple presentation of interannual trends in monsoon moisture sources it could be close to acceptable but it makes jumps about the way circulation influences the $\delta^{18}O$ of precipitation, which are simplistic and not likely valid. While indeed circulation influences the monsoonal precipitation, continental recycling, local amount effects associated with convective processes, and seasonality ALSO influence the $\delta^{18}O$ of the precipitation and cannot simply be ignored. My opinion is that the "proof" the authors are seeking cannot be reached with the methodology used. As opposed to an empirical approach, I suspect the authors would need to use an atmospheric model, where the various processes that influence the $\delta^{18}O$ can be held constant allowingthe influence of the different processes to be isolated. Similar perhaps to the approach taken by Pausata et al., 2011.

Response: We partly agree with these comments. Actually, besides circulations other factors mentioned by the reviewer may greatly influence the $\delta^{18}O$ of precipitation and speleothem, all of which, indeed, should be considered in order to avoid misjudgment. Meanwhile, it has also been recognized that, in many cases, one factor could play a major role. For example, the annual time series of precipitation $\delta^{18}O$ in Hong Kong shows very poor correlations with the precipitation amount ($r = 0.05$, i.e. no amount effect) and the temperature ($r = -0.04$, i.e. no temperature effect), but robust correlation with the trade wind index ($r = -0.72$, i.e. very strong circulation effect). Therefore, we should give a comprehensive analysis in the revised manuscript.

We also appreciate the reviewer's suggestion of using an atmospheric model to analyze the $\delta^{18}O$ even although another Chinese group has been doing it (Zhang X. et al. Procedia Environmental Sciences, 2011, 10B: 1601–1612). Their result shows that there are some discrepancies between GCM simulation and GNIP survey. Compared with actual survey data, the four GCMs underestimate the $\delta^{18}O$ in mid-high latitude inlands. What we would like to say is that each individual method has its incompleteness, and the correct option is that all imperfect methods could complement each other. We thus still believe that the analysis on ratio of water vapor for revealing the mechanism of isotope effect is worth doing.

Major Comments (some reiteration of statements above):

1) I was not convinced from the Supplementary figure that d18O in many of these stalagmites was different in 1955-1975 relative to 1975-1995. In order to make this case, the authors need to use statistics and show the population in the first set of decades different than the population in the second set of decades. Also, this analysis would need to consider age model error and analytical uncertainty.

Response: Agreed. In the revised manuscript we have used statistics to verify the trend of the stalagmite $\delta^{18}O$ (not a shift yet) and also considered the age model error and analytical uncertainties.

2) Perhaps this is just a rhetorical issue, but why is BoB considered "remote" and WNP "local"? The distances seems about the same visually.

Response: This is a question worth answering carefully. Recognition of "remote" or "local" is certainly not confirmed by eyes. Based on back trajectory analysis with the HYSPLIT model, Zheng et al. indicated that the differences of $\delta^{18}O$ values were determined by different water vapor transport paths for Yunfu city (22˚22'–23˚19'N, 111˚03'–112˚31'E): relatively higher $\delta^{18}O$ values of the water vapor located in the South China Sea and the West Pacific Ocean, whereas relatively lower $\delta^{18}O$ values of the water vapor advected from the India Ocean and Bengal Gulf (Environ Sci 2009, 30: 637–643). In addition, according to isotope gradient analysis Liu et al. inferred that, for most areas of the monsoon region of China (except for a few areas in the west part of the region), the Pacific Ocean supplies the local water vapor enriched in ^{18}O

and the Indian Ocean the distant water vapor depleted in ^{18}O (J Geogr Sci, 2008, 18:155–165; Chin Sci Bull, 2010, 55:200–211). These literatures, therefore, should be cited in the revised manuscript. In addition, the following example may be more attractive:

Occurring in August of 1997, typhoons Victor and Zita, the only two tropical cyclones attacking Hong Kong that year, led to the highest monthly rainfall (829 mm) with monthly $\delta^{18}O$ valued at –5.61‰ (VSMOW) in Hong Kong. Moreover, it has been confirmed that tropical cyclone rainfall tends to be more depleted in the heavy isotope of oxygen (^{18}O) than typical summertime low- to mid-latitude rainfall (see Kilbourne K. H. et al., AGU 2012 Fall Meeting, PP33A-2104; or Frappier, A. B. Geochem. Geophys. Geosyst., 2013,14:3632–3647 for details). Then the rainwater $\delta^{18}O$ value in August was supposed to be extremely low if taking the double negative-bias resulted from amount effect as well as typhoons into account. In fact however, comparing the case of August with adjacent months, the rainfalls were 764 mm with monthly $\delta^{18}O$ valued at –7.51‰ (VSMOW) in July and 233 mm with monthly $\delta^{18}O$ valued at –10.21‰ (VSMOW) in September, respectively. The typhoon brings water vapor from, no doubt, the Pacific Ocean to the monsoon regions of China. Thus, here is only one explanation that the water vapor from the Pacific Ocean (local) is more enriched in ^{18}O than other oceanic sources for at least southeast China.

3) BoB provides about double the flux as the other sources. Therefore, from a mass balance consideration its influence would completely override the influence of the other sources unless of course the isotopic composition of the different regions produces moisture that is radically distinct. There are various datasets such as HDO from satellites and many isotope-enabled GCM simulations that should be able to show whether the moisture from the BoB is really different from the moisture from WNP and SCS. If the moisture sources from the three regions are similar (and I expect they are) than subtle shifts in the contribution of WNP and SCS should have only a tiny impact on the $\delta^{18}O$ of the precip.

Response: We partly agree with the reviewer's comments and, moreover, we would like to discuss this issue in depth. The fact that Indian Ocean provides about double the vapor flux as the source from Pacific Ocean has been shown in our paper and other literatures. However, it is also true that Indian monsoon is not the only water vapor source that affects Chinese precipitation $\delta^{18}O$. Based on the linear isotope mass balance mixing model (Schwarcz, J Archaeol Sci 1991, vol.18, pp261–276; Phillips, Oecologia, 2001, vol. 127, pp166–170), a formulation to partition the isotopic contributions of two sources (a, b) to a mixture (m) is

$$\delta_m = f_a\delta_a + f_b\delta_b$$

$$1 = f_a + f_b$$

where f represents the proportion of mass, the subscripts a, b, and m represent two sources and the mixture. It is obvious that the influence of either vapor source a or b to δ_m could not be ignored even if one of them is one third of the other. For example, supposing a quarter of the water vapor from Pacific Ocean (f_a) with a mean $\delta^{18}O$ value of -5‰ (δ_a), three-quarters of the water vapor from Indian Ocean (f_b) with a mean $\delta^{18}O$ value of -8‰ (δ_b), then the $\delta^{18}O$ value of mixture (δ_m) is -7.25‰.

Another example comes from Delingha, a city located in the northwest China. At that region the water vapor comes mainly westerly, but the minor part transported by easterly wind also affects the precipitation $\delta^{18}O$ evidently (see Figure 1 below).

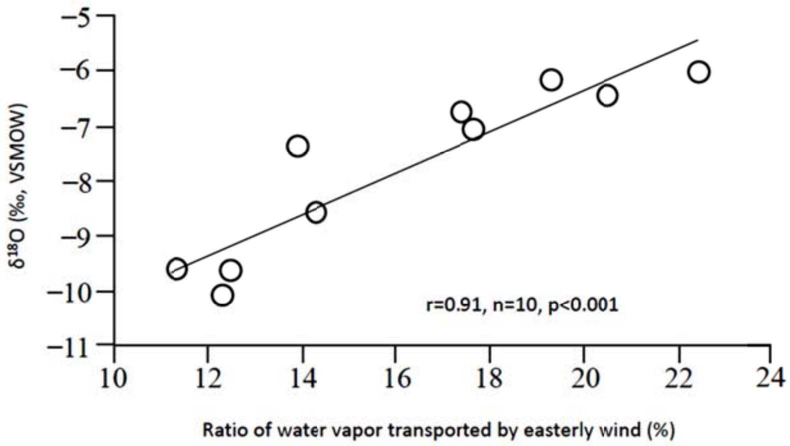

Figure 1 Relationship between ratio of water vapor transported by easterly wind and annual weighted mean $\delta^{18}O$ in precipitation in Delingha (37˚22’N ,97˚22’E ,altitude 2981m) from 1992 to 2001 (after Li Z. et al., Earth Science Fronitiers, 2006, 13: 330-334)

In addition, we agree that the $\delta^{18}O$ value of sea waters between Pacific Ocean and

Indian Ocean has little difference (about -1.5‰~1.5‰). Therefore, there is almost no "source effect" in the precipitation $\delta^{18}O$ we discussed. Instead of that, a significant difference between the $\delta^{18}Os$ of various water sources detected by a station due to its position or distance relative to different oceans. We realize that the upstream-depletion effect (as shown by Pausata et al., 2011) and the circulation effect are somewhat contradictory, but more likely co-exist. The GNIP data obtained from special years could give us the opportunity to test this hypothesis. For example in 1997, Indian monsoon rainfall is 1175mm, and the anomaly is +90mm (reference to the average from 1871 to 2011). According to the hypothesis of upstream-depletion effect, the Chinese precipitation $\delta^{18}O$, as from the downstream regions, should be negatively biased relative to the normal. However, the observational data shows us a different picture illustrated as in table 1 and figure 2 below. In 1997, we have nine Chinese stations possessing data of precipitation and $\delta^{18}O$ in the precipitation. In addition to Wulumuqi Station in the northwest region, the other eight stations are scattered in the vast eastern monsoon region. It is clear that most stations (8/9, i.e., number 1 through 8 in blue) possess positive biases of precipitation $\delta^{18}O$, and meantime, half of them also possess positive biases in precipitation, which is contrary to the expectation of the upstream effect hypothesis but could be explained by the circulation effect hypothesis.

Table 1 Precipitation (P) and $\delta^{18}O$ in the P from nine GNIP stations in China in 1997

1-1 Haikou precipitation and anomaly 11-month data	1-2 Haikou precipitation $\delta^{18}O$ and anomaly 11-month data	2-1 Hong Kong precipitation and anomaly 12-month data	2-2 Hong Kong precipitation $\delta^{18}O$ and anomaly 12-month data	3-1 Kunming precipitation and anomaly 12-month data	3-2 Kunming precipitation $\delta^{18}O$ and anomaly 12-month data
2188mm +533mm	-5.56‰VSMOW +0.39‰	3344mm +1065mm	-5.58‰VSMOW +1.08‰	1313mm +320mm	-9.78‰VSMOW +0.57‰
4-1 Chengdu precipitation and anomaly 10-month data	4-2 Chengdu precipitation $\delta^{18}O$ and anomaly 10-month data	5-1 Wuhan precipitation and anomaly 12-month data	5-2 Wuhan precipitation $\delta^{18}O$ and anomaly 12-month data	6-1 Lanzhou precipitation and anomaly 8-month data	6-2 Lanzhou precipitation $\delta^{18}O$ and anomaly 8-month data
779mm -10mm	-5.16‰VSMOW +1.83‰	942mm -19mm	-5.37‰VSMOW +1.29‰	232mm -71mm	-4.70‰VSMOW +0.61‰
7-1 Shijiazhuang precipitation and anomaly 9-month data	7-2 Shijiazhuang precipitation $\delta^{18}O$ anomaly 9-month data	8-1 Ha'erbin precipitation and anomaly 4-month data	8-2 Ha'erbin precipitation $\delta^{18}O$ and anomaly 4-month data	9-1 Wulumuqi precipitation and anomaly 9-month data	9-2 Wulumuqi precipitation $\delta^{18}O$ and anomaly 9-month data
323mm -231mm	-7.39‰VSMOW +0.37‰	476mm +17mm	-8.39‰VSMOW +1.57‰	156mm -147mm	-11.33‰VSMOW -0.71‰

Figure 2 Distribution of nine GNIP stations possessing precipitation and δ^{18}O data in China in 1997. The station marked by blue circle possesses positive-bias of annual mean weighted precipitation relative δ^{18}O to average and the station marked by red circle possesses negative –bias

4) A recent paper in JGR by Lee et al., (Asian monsoon hydrometeorology from TES and SCIAMACHY water vapor isotope measurements and LMDZ simulations: Implications for speleothem climate record : : :) show that vapor traveling over Asia does not follow a Rayleigh distillation pattern but rather becomes enriched as it travels into the continent. This challenges the simple notion that "local" vs. "remote" moisture sources are necessarily isotopically distinct. Please discuss this paper and its implications for your interpretation.

Response: Yes, We also note that the Lee et al.'s result seemly refers an "anti-continent effect", and we have two comments for this. First, the Lee et al.'s result reveals that re-evaporation of raindrops below cloud base due to the dry conditions in the arid inland leading to a positive-bias in precipitation δ^{18}O, which is an important complement to the Rayleigh distillation theory. Second, "anti-continent effect" refers to a spatial pattern, but the ratio of local to remote sources of water vapor is related to circulation varying in time, which could co-exist with other effects. See the example shown in figure 1 above, the precipitation δ^{18}O of station Delinghua is distinctly affected by the changing ratio of different sources of water vapor although it is located in the northwest China (about 700 km west of Lanzhou).

5) All trend analysis of vapor sources and vapor source ratios need to be done with considerations of statistical confidence. I am not convinced visually of the presence of trends in these datasets.

Response: Agreed and we have done these in the revised manuscript.

6) The abstract needs to be rewritten. Within the first few sentences the reader is accosted with at least 12 acronyms. By the end, I was completely lost. I realize that acronyms are a necessary evil but at least for the abstract distill the main points of the paper in a way that a reader can immediately appreciate the significance of the study.

Response: Agreed and we have deleted 8 acronyms and rewritten the abstract.

7) This is similar to #1 above, stalagmites integrate water in the karst and discrete sampling also integrates. So some consideration of this needs to discussed when arguing for the presence of decadal-length shifts. I was surprised for example, that in the composites in Figures 4-6, there were similar numbers of extreme positive and negative events coming from 1955-1975 as from 1976-1995. This made me think that because stalagmites are integrators, that perhaps instead of discussing changes in trend, the authors should calculate total (as opposed to averaging) grams of H_2O from BoB, SCS and WNP in these two different decadal windows this would better represent the influence that extreme years might have on the isotopic values.

Response: Agreed. With consideration of the different resolutions of stalagmites data in the revision, we focus only on their trends with statistical confidence as the reviewer suggested above instead of decadal shifts.

8) In Figure 4a none of the differences are statistically different so it is not really appropriate to discuss the differences as though they are meaningful.

Response: Agreed and we have removed figure 4a.

9) It would be valuable to in some way diagnose whether NCEP Reanalysis is actually

capable of capturing the monsoonal moisture fluxes. The best way I can imagine doing this is to do the same calculation with an independent Reanalysis dataset such as JRA, ERA, or MERRA. These datasets do not go back to 1948 but you could see if they produce the same results over the overlapping period. A comparative analysis of Reanalysis monsoon moisture fluxes alone would warrant a valuable manuscript.

Response: Agreed. We have selected ERA40 (1957.09-2002.08) to repeat the work. The results from ERA40 are similar to those from NCEP-NCAR reanalysis.

10) There are a lot writing errors throughout.

Response: Accepted and we have improved the writing throughout.