Taft et al present a novel source of information about past monsoon variability - variations in oxygen and carbon isotopes from gastropod shells. Unlike other proxies, which average over decades or centuries, this proxy has the potential to provide subseasonal resolution about the monsoon. My comments fall into two general categories: improving the paper's organization and clarity, and improving the interpretations of climate from isotopes.

General comments on paper organization and clarity:
a) Some sections could be shortened to improve readability. Examples of places to reduce the details and make the main points clearer are: lines 89-117 (focus on summarizing regional patterns rather than providing description of lots of lakes individually),

Our weather and climate archive Radix lives in lakes and thus we preferably address former lake studies from four lakes of the western Tibetan Plateau in which the authors have presented data we consider relevant for the interpretation of our results. These references show that there are some similarities across the region and we agree that we should better emphasize these (and we will try so), but on the other hand it also shows how differently the lakes developed. Tso Moriri and Tso Kar e.g. are located in neighboring valleys. While Tso Kar was about a hundred meters deep during the Mid-Holocene, it is a shallow pond nowadays. On the other hand Tso Moriri is still ca. 100 m deep and the water level has dropped for a couple of meters only since the Mid-Holocene. This is due to the different drainage basins which is much larger in the case of Tso Moriri and the elevation of the surrounding mountains is also higher there, etc. On the other hand Tso Kar was stronger influenced by tectonics. We will rephrase this paragraph to make clearer why we mention these studies and try to shorten the text.

lines 144-193 (focus more on details relevant to this study),

We consider the information on the hydromorphology of the drainage basin and other details of the study area important in order to better understand regional processes potentially affecting isotope values. It makes e.g. a difference if there are glaciers in the catchment or not, etc. The size of the lake area e.g. provides information about potential moisture recycling, etc. We will shorten this paragraph and put a stronger focus.

lines 468-500 (combine with similar information in section 4.3.2),

These two paragraphs provide different information. The mean values average over the life-spans of the gastropods and primarily do not reflect seasonal changes while the sclerochronological isotope patterns do so. We believe the results are more transparent as it is arranged but will make an attempt to integrate these different categories.

lines 562-633 (it is unhelpful to provide one entire paragraph for each shell; one paragraph summarizing the main similarities and differences would be preferable).

We believe that it is essential to address each shell separately. Otherwise the reader may not be able to follow our reasoning. We understand from your comments that it is anyway not easy to follow the discussion and that you consider over-interpretation in parts. You also suggest that presentation of published data (see below) may help to better understand our conclusions. Against this background, we prefer not to shorten the discussion but make it *even more transparent by adding information from modern shells as outlined under the paragraph below, "Climatic interpretations of isotopes: b)"*

b) Are Table 4, Section 3.2, and Section 4.2 regarding mollusk ecological traits necessary? They don't seem to contribute to the main goal of the paper.

This table is important because the interpretation of isotope patterns to some degree depend on the habitat situation of Mid-Holocene Radix. The data confirms on the one hand that the nowadays saline pond was a freshwater system and thus the water residence time was relatively short and on the other hand that Radix lived in shallow water which is important regarding the signal strength. If it lived several meters deeper some buffering effects of the hydro-climate signals have to be considered. If you do not analyze the mollusk assemblage but take the ecology of Radix alone it also could have lived in a mesohaline environment and in deeper water. We will rephrase section 4.2. to make our point clearer.

c) Much of the discussion is more appropriate for a results section. For example, section 4.1 could be added to section 3.1. Lines 562-633 could be moved to section 3.3. The main things that should remain in the discussion are the inferences about climate.

We can do this, however we followed the concept of not mixing results and interpretation and need the comment of the editor here.

d) The paper would benefit from editing throughout for proper English usage.

As some of us have lived in the USA for several years, we were confident that our English usage is sufficient. We will ask a native speaker to edit the manuscript.

Climatic interpretations of isotopes:
a) The d18O and the d13C proxies are very complex with multiple competing influences, as the authors describe on lines 512-560. Thus, there are many different ways to explain a particular isotope excursion. Having both d18O and d13C does not necessarily help, either, because d13C is so complex. For this reason, some of the detailed interpretations presented on lines 562-633 regarding certain excursions being due to soil inwash, for example, or others being due to meltwater pulses, etc., seem very arbitrary and overinterpreted. For one more specific example, the authors generally consider periods of low d18O variation to be ice periods, but on line 571-573 a similar low-variability period is considered "too long" to be due to ice and is assigned another cause (even though it is impossible to say anything definitive about how many weeks or months a particular part of the shell spans). A simpler, more defensible, and more objective approach might be to report on several relevant metrics (like mean, standard deviation, and range) and compare how these vary from modern to Holocene.

We believe we can overcome these problems by following your suggestion (see below) to include published data of modern Radix shells from several lake and climate settings (Taft et al. 2012, 2013). It will become evident then that e.g. inwash of carbon dissolved in soil is not an over-interpretation but there is a clear correlation of negative d13C excursions and stronger (monsoonal) rainfall. The modern shells were collected by ourselves and therefore we knew quite exactly the life-spans and could relate it to synoptical data of particular years *allowing interpretations such as ice cover period, etc. We did not want to overload the paper with already published data on modern shells but we see your points and follow your idea to include these. It certainly will make it easier for the reader to judge on our interpretation of Mid-Holocene isotope patterns.*

b) I wished for more isotopic data from modern shells to compare with the five Holocene shells, in order to more quantitatively describe the modern-Holocene differences. At one point, the authors give the mean of two modern shells and this is useful for a very first order comparison of the hydrology (but one that could also be explained by changes in lake water residence time that we already know about between early/mid Holocene and modern). The stated goals of the paper are to look at more of the subseasonal signal, though, and for that we really need to compare with sub-seasonal signals of modern samples. Perhaps the authors have published such data in other papers. In that case, it would be useful to present it here again for comparison.

The Nama Chu pond is too saline nowadays that Radix can live there. Therefore we could not apply a direct modern analogue but used two modern shells from neighboring Nyak Co (eastern Bangong Co). It is a very good idea to present selected published data again. Particularly because most of the published isotope data are from modern shells and thus could be related to synoptical data, it will likely help to better demonstrate the potential of the archive.

c) Given the large interannual variability in the monsoon region, it is unclear that 5 years is enough to truly give a good sample of Holocene monsoon climate. This is unfortunate, because I know how much work goes into sub-seasonally sampling even one shell! But it is important to recognize what these results do and don't tell us.

Papers have been published in high-ranked journals which present one or two sclerochronological isotope patterns in order to open some weather window of the past. You are completely right that more is mostly better but we believe that we found a good balance and that the data presented allows us to make the conclusions we did. Again, this will hopefully become clearer when we have included isotope patterns of modern shells from the Tibetan Plateau.

d) I was confused about the conclusion that the precipitation is not continuous, but in pulses. Generally, precipitation does occur in pulses (storms), even in locations within the core monsoon. Particularly in this dry part of the world, it doesn't rain every day, but certain weather systems will deliver moisture from time to time. So, this conclusion seemed obvious and non-consequential. The authors mentioned that lakes on the eastern Tibetan Plateau reveal single extended events (line 640-641), but there are other factors such as significant groundwater inflow that could smooth a d18O series.

With pulses we do not address single weather events but rather a sequence of cloud bands which rained out over the research area during a certain period. This you may either call a short monsoon season (single "pulse") or a double-peak monsoon season. We assume that between two such periods ("pulses") the cloud bands rained out further south or the rain was too weak a signal to be archived in the shells. Under modern climate conditions, with Bangong Co located at the northern limit of monsoon moisture, rain periods strong enough

to be archived in the shells may occur only exceptionally (Taft et al. 2013). In the Mid-Holocene shells, however, we could identify monsoonal rainfall during two periods which means that the rainfall was significantly stronger then. The already published data e.g. on the eastern plateau lakes Bangda Co and Donggi Cona will be presented here again (following your suggestion). These graphs show that although the monsoonal rainfall derives from many single weather events there is an overlying pattern defining a single monsoon season (this is not in "pulses"). The rate of summer growth of Radix shells allows for a maximal resolution of ca. 1 week (for temporal resolution see f) from lines 556-560) allowing some averaging of storm events. The isotopic signal of the rainfall also depends on the humidity. It differs strongly between early and late monsoon season e.g. (Taft et al. 2012,2013).
You are right that other factors than rainfall such as groundwater inflow can influence the the isotopic pattern. At this elevation groundwater derives either from rainfall (thus providing a good average rain signal) because the permafrost does not allow the water to penetrate to deeper soil or rock layers and on the other hand thawing of permafrost itself produces groundwater. Regarding our interpretation of isotope patterns we are fully aware that several hydrological processes interact. Our interpretations however put the focus on the dominant factor likely superimposing others.
We suggest to rephrase these parts of the manuscript that the reader can easier follow our interpretation.

e) It was also unclear how the conclusions about the northern boundary of the monsoon were reached. How can you tell this from one lake? You just know that this particular lake received monsoon moisture both today and in the early/mid Holocene. I think it is very hard to say, based on difficult-to-interpret isotope data and only five years worth of data, that this lake received more monsoon rainfall during the Holocene than today. It seems like a transect would be needed to really answer this question.

We could find in modern Radix shells from the Tibetan Plateau (Taft et al. 2012, 2013) that weak rainfall cannot be identified and is not archived respectively. Drizzle e.g. is influencing the humidity but is too little an amount to create a signal in the shell of a gastropod which lives in water. The inwash of soil with dissolved terrestrial carbon is also possible only by strong rain (see also comment above about relation of rainfall and soil inwash identified in isotope patterns). We cannot tell however how strong it exactly has to be. It may vary from system to system too. We can see a clear difference between modern and Mid-Holocene isotope shell patterns though. From this we conclude that the monsoonal rainfall was stronger during the Mid-Holocene.

f) Conclusions distinguishing monsoon precipitation from meltwater influence do not seem supportable because the isotopic ranges for these sources are not obviously differentiable (according to lines 512-527, estimates for monsoon precipitation and snowmelt are both around -14 per mil).

We do not identify the snowmelt solely by d18O per mil but in the context of the (sub-) seasons archived in the shell. Ice cover is nowadays from November to April. Snowmelt becomes significant from May and peaks in July. The general pattern was likely similar during the Mid-Holocene. We assume that it can be ruled out that the monsoonal rainfall has reached the area directly after the ice cover period in May but that the meltwater was *dominant in May. Convective rainfall could (and did) occur already in May when evaporation of moisture from thawing soil and open lake surfaces became effective. Later in the season the meltwater inflow was superimposed by increasing (insolation) evaporation. Monsoonal rainfall with d18O similar to meltwater likely did not reach the area before July. From this seasonal chronology we believe we can differentiate between the two moisture sources although the d18O values were similar. Additionally, however, we argue that "stronger" monsoonal rainfall triggers inwash of terrestrial carbon and we thus can see d13C excursions to more negative values approximately synchronously to d18O excursions. This negative d13C peak does not occur during May meltwater inflow.*