General

The study presents a 100 year record of water stable isotopes derived from combination of several alpine shallow cores and a deep ice core collected at Mt. Elbrus in the Caucasus. Thanks to the high annual net accumulation rate at the site, high temporal resolution could be achieved, allowing obtaining a seasonally resolved data set. Meteorological data, reanalysis temperatures, GNIP isotope data and isotope modeling results as well as atmospheric circulation indices are used to investigate the regional climate and for the discussion of the ice core record. The study concludes that for the ice core site the isotopic composition in summer is related to local temperature whereas in winter it is modulated mainly by large scale atmospheric circulation.

Clearly this is a very valuable data set from a region with a lack of high-elevation meteorological data and therefore deserves publication. The drilling location is characterized by limited surface melt and the ice core(s) analysed are of high quality. Both of which emphasizes the presented records with clear seasonal variations to be useful for their interpretation as climate proxies. Because of the clear seasonality, the dating by annual layer counting is very convincing. However, and here I have to largely repeat the criticism of all three referees who reviewed the manuscript after its first submission, the applied separation into seasonal data, the applied statistical methods (and the lack of some of them) together with the imprecise writing (partly also related to language) does not allow to convincingly support the conclusions made. In their reply, the authors did address the concerns being raised in the previous review but their argumentation, resulting in minor changes only (not major as requested) is not very convincing either. Until the still persisting main issues are solved it (still) makes not much sense to provide detailed comments regarding interpretation and conclusion of the final results as those may (or may not) change. Instead, I once again try to summarize the main concerns adding another level of details and ideas how they might be addressed. Hopefully this will provide help for improvement of this potentially valuable manuscript. In summary, the current manuscript still requires major revision.

Because the manuscript uploaded after the open discussion seems not to be the revised version (File Upload 22 Nov 2016), my review refers to the "track changes version" attached at the end of the Author's Response file (also the line numbering). In my review I will also discuss the Author's response to the *Referee comments (in italic)* made during the open discussion.

<u>Separation into seasonal data:</u> Main point of concern.

Only once this issue is properly solved, the points discussed later on should be addressed because some of the current results/values might change significantly (not necessarily though).

Referee 1 wrote: "…is conducted by implementing a threshold (average d18O value of -15.5‰ for the entire record), thereby inherently presuming a d18O-temperature relationship and the absence of a trend. This introduces a circular argument when examining the temperature dependence of the resulting warm and cold season record."

The main point here is the "circular argument". Even when the approach how the separation is performed (d18O threshold) may lead to seasonal separation in agreement with reality we cannot be sure if this is the case unless there is independent confirmation. More details will be provided in the following.

In their reply, the authors argue: "…, we think that the proposed method of dating when the border between warm and cold seasons is the 100-years mean value is the best one for this very ice core." and later on "We think that the annual cycle of the isotopic composition is influenced by local temperature while interannual variations depend on the other factors."

Well, the first point is not really an argument whereas the second point is an assumption. This assumption defines the outcome and thus the outcome cannot be interpreted as a climate signal (circular argument).

It is very likely that the annual cycle of the isotopic composition is influenced by local temperature as indicated by the clear seasonal variation in the signal. Interannual variations likely also depend on other factors but at least partly they may be influenced by temperature as well. How much these other factors and temperature have contributed in the past might have changed over time and is a focus of the study. By using the 100 yr mean the possibility of longterm trends in annual T (e.g. on a decadal scale) is neglected. Instead such changes observed in d18O are assigned either to changes in cold/warm season temperatures or changes in seasonal accumulation (or a combination of both). This is much less complex for annually resolved data. For those, changes in accumulation do not depend on any initial assumption but can be discussed directly. For annually resolved T a complication because of a potential shift in the seasonal p distribution remains. However, this can be estimated by either assigning the observed increase/decrease in accumulation fully to either of the seasons. What would the expected shift in cold/warm season d18O be (e.g. more acc in winter results in lower d18O for the cold season)? What does the reanalysis data suggest to which season the change in accumulation should be assigned? Can the observed decrease in winter minima (e.g. depth around 65-100 m depth) be explained by this or do they indeed suggest that winters during that period were indeed slightly colder? Could it be a result of increased sampling resolution for this
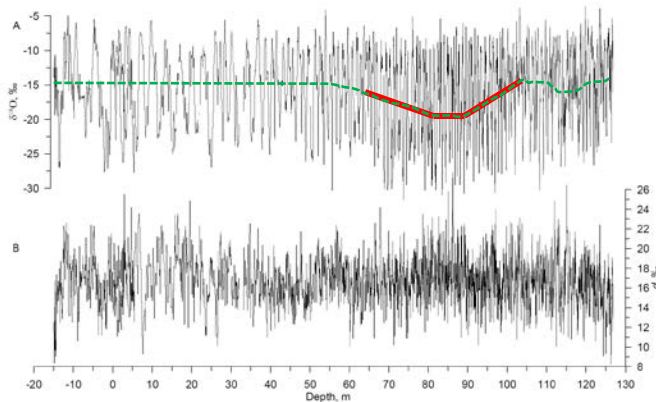
period (higher resolved winter data resulting in a less smoothed winter signal → lower minima)? This should be discussed and for this and the above reasons I strongly suggest including discussion of annually resolved data which currently is completely ignored. Also see comments in the following.

…further in the reply: "Accumulation at the drilling site has been investigated sporadically (see review in Mikhalenko et al., 2015). We cannot use the meteorological observations from the nearest weather stations as these stations situated at sufficiently lower elevation and belong to two different groups as discussed in section 3.1. The ice core is the only source for the information about the seasonal cycle of this parameter."
I agree about the meteorological data. However, there should be regional reanalysis (or modeling) data available which at least might give some indication of potential changes of the seasonal precipitation cycle. To which extent do those agree/support your observations in the ice core on an annual and seasonal scale (see comment above)? Please discuss.

…and further: "In order to better illustrate the dating methodology we will add the ammonium concentration and dust concentration profiles to Fig. 3. Layers with the high dust concentration have been precisely dated by Kutuzov et al. (2013) for the 2012 ice core. Their results show that the separation of the core into a warm and cold season part using the average value of δ18O is appropriate for this drilling site at least for the period from 2009 till 2012 that was investigated in the paper."
So the solution to avoid the circular agreement is presented right here by introducing the chemistry data as an independent parameter. Chemistry largely relates to seasonal transport (vertical, convection) and seasonal emission (e.g. NH4+). Accordingly it is obviously a good choice to be used for separation into seasons. For the data in the cited study as well as for the period shown in Fig. 3 for which the d18O mean is very similar to the 2009-2012 period and actually also very close to the 100 yr mean of -15.5 permil, this is convincing and suggests that the chemistry records could indeed be used for separation into seasons (or d18O if only this period was considered). So why not just use d18O entirely as suggested by the authors? First because of the circular argument and second because of the period 2009-2012 likely not being representative for the last 100 years. In other words, the approach using a mean d18O value is especially problematic for the depths (i.e. periods) where d18O has a strong trend and differs significantly from the mean (see manuscript Fig. 2 below with these regions marked in red).

To clarify once more the problem of circular agreement on can use the above figure for illustration: if for the separation into seasons a straight line through the data at the value of -15.5 per mil is drawn one can imagine what the outcome likely will be. For the red marked region (around 65 to 105 m depth) the warm season (denoted as summer in the manuscript) d18O values will roughly be similar as for the other depth intervals because the maxima do not vary much. The cold season d18O values on the other hand will become slightly lower (see minima). At the same time the accumulation for the warm period will become smaller whereas accumulation will become bigger for the cold period. Any outcome will thus be defined by our initial assumption and accordingly cannot be interpreted as the reflection of a climatic signal.

Not knowing anything in the first place, looking into annually resolved data (see comments before), may provide us some initial, record based information of what really might have happened during these periods. Was there an increase/decrease in annual precipitation? What is the estimated potential effect on d18O if this change is either fully related to an increase/decrease solely in one season? Could there be a potential effect due to the sampling resolution if the accumulation increased/decreased strongly during one of the seasons (e.g. more pronounced minima)? All of those possibilities could be discussed based on the available station or re-analysis data (Which assumptions are most likely based on those independent results?). As pointed out earlier, whereas a change in annual accumulation can directly be extracted from the ice core record (after corrected for potential thinning) a decrease in annual d18O might not be indicative of a decrease in annual temperatures since a shift to cold season precipitation could have this effect and the observation would accordingly be unrelated to temperature. Using the chemistry (especially NH4+ with main emission in the warm season) already could solve one part of the puzzle, namely the question if the observed increase/decrease in accumulation is related to the cold or the warm season (or both). With this information, one can already come up with a better estimate of the effect of precipitation shift on d18O. Only then, one might start discussing the seasonally resolved data.

Reviewer 1 also wrote: "…the question is if you could investigate a longer time period (potentially showing a trend in temperature) and longerterm averages to smooth the effect of year-to-year shifts in precipitation/accumulation."
The authors showed Fig.2 with a linear trend as a response. I do not think this is what the reviewer meant and accordingly the correlation analysis with 3-, 5-, and 7-years running means is out of context here (and should be deleted again). Instead the reviewer's idea seems to be to reduce the high frequency signal (i.e. sub-decadal variations). Such a strongly smoothed signal could then be used as the threshold instead of the 100 yr mean to distinguish between the warm/cold seasons (see again manuscript Fig. 2 above, hand-drawn green dotted line). I am not supporting this idea as one assumption would just be replaced by another one and the circular argument would still persist. With this approach the question would be what the variations in this low frequency signal are related to? Are those decadal T variations or changes in climatic pattern? Because d18O is used as the threshold parameter one could not distinguish the two. Again, an independent parameter such as the chemistry data to split into seasons should allow overcoming this problem.

In summary:

The annually resolved data should be investigated first and based on the thereby gained information one can start interpreting the seasonal data which has to be derived by splitting the years based on an independent parameter (e.g. chemistry). Also for the chemistry data a threshold should be defined to separate between cold/warm (or summer/winter) seasons (or at least to indicate the onset of the seasons because in some cases one might see levels below threshold mid-summer e.g. due to a dilution effect in a high precipitation event). Preferentially multiple species (e.g. $NH4+$, dust/$Ca2+$) are used to overcome potentially unclear separation for some years. Be aware that since e.g. $NH4+$ likely has a trend due to an increase in anthropogenic emission, this trend (not necessarily linear) has to be removed first (or considered for the threshold).

To avoid the circular argument, the authors should use the chemistry data for separation. Another option would be the approach chosen by Mariani et al., 2014 where the record is simply divided into 12 equally spaced bins between peaks (mid-summer; or dips accordingly mid-winter). This approach however assumes equal distribution of annual precipitation. However, by selecting only summer (JJA) and winter months (DJF), the so introduced potential bias/error (if present at all considering the S precipitation pattern) is reduced. It could be estimated if assuming the N or S pattern instead (probably in the order of 10-20%). Another approach (heavily smoothed signal as the threshold instead of the 100 yr mean) was suggested by Reviewer 1, which however also has some caveats as pointed out before.

Other major comments:

Seasons and summer winter definition:

The terms summer and winter are used for the ice core data separated into two seasons (e.g. in the Abstract line 404). Since the year is thereby divided in two seasons only this can certainly not be correct. The authors do give a definition of summer (May-Oct) and winter (Nov-Apr) rather late in the manuscript. Nevertheless, this definition is very uncommon and certainly extremely confusing. I suggest sticking entirely to the term warm/cold season with this term being defined in the very beginning of the manuscript (indicate months belonging to the respective seasons).

Correlation:

Throughout the manuscript it is difficult to keep track in what resolution the correlation analysis were performed (annual, seasonal, multiannual/smoothed data?). With at least the numbers of years included in the Tables this has already been slightly improve in the new version. Still it is unclear. I thus suggest to include the time period (19xy – 20zx?) and number data points (n=?) instead. This information should also be given in the text.

Line 586 ff (new section 2.3 Statistical methods):

The calculation of the degree of freedom for the smoothed data set is not correct. The estimate of Reviewer 3 was much better. See e.g. Friston et al., 1994 and 1995; Worsley and Friston, 1995. I will try to give a more intuitive explanation of the results therein here:

Consider a series of n independent observations of a population of mean m. The variance of the population is given by $\sigma^2 = \frac{\sum(X_i - \mu)^2}{N}$ which is best estimated by the sample variance $s^2 = \frac{\sum(X_i - \bar{X})^2}{n-1}$. The denominator in this expression is the number of degrees of freedom of the sample, and is one less than the observations, since only n-1 points are needed to describe the sample, the "last" point being determined by the mean. Now if the data set is smoothed with e.g. a 3 point running filter such that $X_n = (X_{n-1} + X_n + X_{n+1})/3$. The sum of the square deviations from the mean of the data is now (on average) three times less than that of the unsmoothed data. This means that the population variance is now best approximated by $s^2 = \frac{\sum(X_i - \bar{X})^2}{(n-1)/3}$ and implying that the smoothed data set has (n-1)/3 degrees of freedom. Applied to your case with a 11 yr running mean this results for the period 1994-2013 with df=(10-1)/11=0.82 and for 1914-1928 with df1=(15-1)/11=1.27. Since you then combine these two data sets for the correlation analysis the degree of freedom for this combined set is df2= ((10-1)+(15-1))/11 = 2.09. Now for the correlation analysis this results with **dfTotal**≈2*df2-2= **2.2**. For the period 1929-1993 the dfTotal is 2*((65-1)/11)-2≈**9.6**.

As a consequence the significance levels of all correlations using smoothed data have to be reconsidered. Considering this, also the newly added panel in Fig. 11 does not make any sense as in the sliding window the number of data points is even further reduced. It should thus be removed as it does not contain any useful information.

2.1.5 Diffusion of stable isotopes
Line 565-566: "Moreover we would find a positive correlation between accumulation rate and seasonal amplitude of δ18O."
I do not see why you would expect this to be correlated under this assumption. I think what is meant is the actual layer thickness and not the accumulation rate?

3.1 Regional climate
Line 600-604: "Meteorological data depict large regional variations in the seasonal cycle of precipitation. To the south of the Caucasus, there is no distinct seasonal cycle (Fig. 4a), showing the climatology for the Klukhorsky Pereval station. In fact, the Klukhorsky Pereval station is situated north of the Main ridge, but in terms of the seasonal cycle of precipitation it undoubtedly belongs to the southern group. But we are nevertheless using this station as an example because of the uninterrupted record of temperature and precipitation for the 1966-1990 period."
The way it is written here the choice is not very convincing. Reading further on in the manuscript I agree that this seems to be the best choice. But this should become clear at this point already. Also, in the new Fig.1 this station seems to be S of the main ridge?

You are in the fortunate position to have station data both from the north (2-5) and the south (most relevant probably 9 and 10, maybe also 1) as well as high elevation station data for both sides (N: 6,8 and S; 7). As a further plus, the later 3 are in very close proximity to the drill site. I suggest to show the precipitation distribution for all station data (at least in the supplement) and to discuss the patterns according to the groups (N, S, high elevation with N and S indicated) with the final conclusion why this station was chosen. Also see next comment.

Line 606-609: "Moreover, the annual precipitation rate to the south of the Caucasus is much higher than to the north. For example, the typical annual precipitation rate to the north of the Caucasus at the altitude close to the sea level is 500 mm per year, while to the south of the Caucasus at the same altitude it is about 1500 mm. The amount of precipitation in the region is affected by the altitude and the distance from the sea shore."
Line 616-619: "For precipitation data, available in this region since 1966, we considered two different stacks (fig. S4), separating the stations with a distinct seasonal cycle from those where no seasonal cycle was identified for precipitation rates. We coherently used the reference period from 1966 to 1990 for normalization for both precipitation rate and temperature."
Presentation of
   a) Accumulation:
All this information is almost entirely lost in the way Fig. S4 is presented.
1) It is not indicated to which stations the purple lines belong.
2) Because being normalized the absolute values are not visible.
3) The effect of altitude and distance from the sea is not visible since only the stacked record is shown and shown as normalized values.

I suggest following the example in Fig. 8 of Mariani et al. 2014, including all station data on the absolute scale and the altitude indicated behind the station name (one could even think of an additional scatter plot to show the effect of altitude and distance from the sea, respectively). By doing so, the reader is immediately able to visually see all statements made with the additional information about the amplitude of the variations and correlation (visual) between the stations. Since you also discuss seasonal data it would make sense to do provide figures for annual and seasonal values (if the fig does not get too complex, maybe they can be combined).

   b) Temperature:
The above generally also applies to the temperature data sets and its presentation in your Fig. 8 (show all stations, not normalized). Again, in annual and seasonal resolution. Considering my previous comments highlighting the importance of discussing annual values first a panel should be added to Fig. 5 for annual resolution.

Discussion of
   a) Temperature:
In the manuscript the stacked record is then used for further discussion. This assumes that all stations show very similar patterns for the respective region (N or S). Indicated by the standard deviation in Fig. 8, this assumption seems reasonable for the temperature. But also here valuable information is lost by doing so. For example, by using normalized values in Fig. 11, the

information of the slope is lost, which is an important value as it is indicative for the relation between d18O and °C. The slope should be around 0.6 (or in the range of maybe 0.4-0.8). Currently a negative slope is found which is however another issue (see comment later).
I suggest to use the high elevation stations only (one of them should be enough) and correct the T for the laps rate to the altitude of the drill site in order to get the most reliable d18O/T relationship (i.e. slope).

b) Precipitation:

For precipitation the variation between the different stations might be larger. Currently this cannot be assessed with the information provided but will become visible with the suggested changes for presentation of the data.
The information lost if using the stacked and normalized data is the amplitude of variability (both inter-annual and seasonal). Also, the elevation effect in total precipitation should be visible between station and ice core data. If not, it should be discussed.
I suggest to also here using the high elevation stations only instead of the stacked record which in fact likely is not representative for the drill site (too much weight is given to the low elevation stations and the N stations). As pointed out in the manuscript Klukhorski Pereval station (based on the current evaluation with r = 0.65 for both seasons) seems to be the best choice (at least for the current evaluation).

Correlation coefficients for annual resolution should be included in Table 4.

Line 652-654: "As an example we show the seasonal cycle of δ18O and d for Bakuriani station in 2009 (fig. 7). This station is the only one in the region for which the whole uninterrupted dataset for one annual cycle is available. The seasonal amplitude of δ18O is about 10 ‰."
In the revised version the T profile is added to Fig. 7. A quick and dirty calculation based on indicated y-axis-range for d18O (-2 to -18) and T (25 to -5) results in a slope of around 0.6 indicative for the d18O/T dependence. This value is as expected. Please re-calculate more carefully based on the data. How does the dependence change if precipitation weighted T is used instead (if available use daily T and p data for the weighting)? The correlation should improve since d18O can only be recorded if precipitation occurs.

3.2 Ice core records
Line 681-684: "Different patterns of inter-annual to multi-decadal variations appear in the instrumental temperature data (see section 3.1) and ice core δ18O records (Fig 5) emerge for winter versus summer. Consequently, we do not investigate annual mean results, and focus on each season."
I do not understand the statement in the first sentence probably because of language. In any case, the motivation to not use annual data is not convincing at all based on the presented data and for several reasons explained earlier. Based on what assumption can you assume that annual data cannot be compared to meteorological data but seasonal data can? It might be that this will be the outcome of the evaluation of the annual data I proposed earlier but until this is discussed and shown properly such an assumption is pure speculation.

The current splitting of the ice core data contains a large uncertainty by itself. Any finding might thus just be a coincidence. By using the annual data first this additional uncertainty is removed which opposite to the authors argumentation above strongly suggests to investigate the annual results first.

In any case, as suggested before, please add results for the annual resolved data to Table 4 and a panel with annual resolution d18O data to Figure 5. In the current version, the annual data in Fig. 8 cannot be compared anywhere with the annual ice core data.

3.3 Comparison of ice core records with regional meteorological data

Line 714-717: "We found no significant correlation between the ice core δ18O record and regional temperature, neither with the reanalysis data, nor with the observation data, when using the whole period. A significant correlation (r = 0.52, p<0.05) emerges for summer data, when calculated for the period since 1984. The slope for this period is 0.25 per mille per °C. We also repeated our linear correlation analysis using precipitation weighted temperature, and obtained the same results."

The value of 0.25 per mil /°C is very surprising regarding the fact that reasonable correlation was found. It is also a little bit surprising that precipitation weighting did not change the slope (although if no seasonal pattern in p exists this seems not unreasonable).

What data resolution has been used for the precipitation weighting of the temperature? Daily, weekly or monthly data (annual data would make no sense)?

Considering the fact no change was observed, I assume the seasonal distribution of p used for weighting was the one derived for the southern stations? From which station (I suggest to use Klukhorski Pereval station only because it shows highest correlation, see comments before)? How does the correlation and slope look like if the one from the N stations is used instead? How do the correlations and slope look like in this case for the annual and winter d18O record? Please redo the analysis accordingly for the entire period and for the 1984-2013 period.

Since precipitation data is shown only from 1966 I assume the precipitation weighting was only performed for this period? Or did you use the monthly distribution derived for the 1966-2013 period also for the period before, assuming it did not change much (if not done already this might be worth trying)? In any case, the information of what has been done is missing now. Please add.

Line 721-723: "Our results are comparable to those obtained in the Alps by Mariani et al. (2014): again, while the seasonal cycle of ice core δ18O appears related to that of temperature, this is not the case for inter-annual variations, driven by other factors such as changes in moisture sources."

It does not seem that the current results are comparable. See conclusion in the cited paper:
"1. The seasonal cycle of temperature is well-captured in both the Alpine ice cores. On a seasonal scale δ^{18}O is thus a valid temperature proxy explaining ~60% of the signal.
2. On an annual scale the high variability of precipitation, especially at high-altitude sites, might considerably bias the isotopic signal. For the glacier site with homogeneous distribution of

precipitation throughout the year the mean temperature signal is still partly preserved also on an annual scale. In the other case with strong intraseasonal precipitation variability, the annual mean of δ¹⁸O was representative only for temperature during precipitation and not for annual mean temperature."

Line 733-735: "The regression analysis showed significant negative correlation between the two parameters. The regression equation for 11-year running means in the 1914-1928 and 1994-2013 differs from the same for the 1929-1993 (see fig. 11 for the correlation plot and regression equations as well as for the sliding window correlation plot).
Based on what criteria can these 2 periods (1914-1928/1994-2013 and 1929-1993) be separated? This seems rather subjective. If looking at the entire period, the correlation would be much worse and the negative slope would not be observed (i.e. both correlation and accordingly the negative slope would not be significant; which is actually also not the case now considering the issue with the correlation analysis of smoothed data pointed out before). Using p weighted data and a different approach for seasonal separation of the d18O (both discussed before) might lead to completely different results anyhow. So please reconsider once the reevaluation is done.
Line 735-737: "The 10-years sliding window correlation…"
Remove (see discussion of correlation analysis).

Line 943 - New (and old) Fig. 3: Why is there a winter and a summer missing around 31 m? Or should the winter around 33 m cover this entire section from around 31-34?

Minor comments:

Abstract - line 403 ff: "In the summer season the isotopic composition depends on the local temperature..."
..and conclusion line 802 ff: "This may explain the significant albeit non persistent correlation of summer δ18O and temperature."
According to the main text this is only true for a certain period (1984-2013)? Please be precise or reconsider the statement.

Line 524-525 (& Fig. S2):
The overlap between the different cores does indeed look very good. Except for the lowermost 2-3 m of the 2013 core with the 2009 core (around 3-7 m depth in Fig. S2). Please comment.

Line 612-613: "The average regional lapse rate was calculated using the available meteorological data. It is minimum (replace with "lowest") in winter (2.3°C per 1000 m) and maximum (replace with "highest") (5.2 °C per 1000 m) in summer (Fig. S3)."
Is this similar for N and S? Are these numbers and Fig S3 for N and S combined or only for one of the 2 regions (or only one station?)?

Line 678-680: "We note that the shallow ice core from the Maili plateau of Kazbek shows the same mean values of δ18O as the Elbrus ice cores during their overlap period. This is a surprise, given the difference in elevation (500 m) and continentality (200 km distance)."
Is this really that much of a surprise? The continentally should make the d18O at Kazbek more negative whereas the lower elevation should make it more positive. In the sum, the two factors seem to cancel out. Can you give some estimates about the size of those two effects and if a 0 sum is reasonable? For the altitude effect, see e.g. Mariani et al., 2014 and references therein.

Line 774-777: "In order to explore the relationships of the Elbrus ice core datasets with the AMO, we used 20-year smoothed data."
I suggest removing this paragraph about AMO entirely. You do show it in Fig 9 and 10 and in some of the tables for comparison with the meteorological data. At this point it does not add anything but takes away from the main focus. Also, by using a 20 yr smoothed record the df is very low for the correlation analysis (<10, see earlier comment) and the result likely not significant anyhow.

Conclusion - Line 789-790: "We found no persistent link between ice cores δ18O and temperature, common feature emerging from non-polar ice cores (e.g. Mariani et al., 2014)."
This is not consistent whit what has been found in the Mariani et al, 2014 paper: See conclusion therein:
"1. The seasonal cycle of temperature is well-captured in both the Alpine ice cores. On a seasonal scale $\delta^{18}O$ is thus a valid temperature proxy explaining ~60% of the signal.
2. On an annual scale the high variability of precipitation, especially at high-altitude sites, might considerably bias the isotopic signal. For the glacier site with homogeneous distribution of precipitation throughout the year the mean temperature signal is still partly preserved also on an annual scale. In the other case with strong intraseasonal precipitation variability, the annual mean of $\delta^{18}O$ was representative only for temperature during precipitation and not for annual mean temperature."

Line 808-810: "The accumulation rate at the drilling site is highly correlated with the precipitation rate and gives information about precipitation variability before the beginning of meteorological observations."
In the current manuscript, the correlation is rather weak and should be changed to "…is significantly correlated…". However, with the current issues this result might change.

Language:

…needs to be improved in general and the writing has to be more precise.
Find some (rather randomly chosen) examples below.

Abstract - Line 396-397: Here, we report on the results of the water stable isotope composition from this ice core in comparison with results from shallow ice cores.

The report is not about the comparison between the ice core and the shallow cores (although the measurements at different labs and with different methods have been compared and the cores have been overlapped). The important part is that these datasets are combined and then the results are compared with the meteorological data etc (see line 25-27). Please reconsider this statement and/or reformulate.

Line 398-399: Dating has been performed for the upper 126 m of the deep core combined with shallow cores data.
Also here this is unclear. The records from the deep and shallow cores were combined and dating then performed on this combined dataset down to the ice core depth of 126 m (i.e. combined depth 126 m + xy m from the shallow cores).

Line 399:
The record covers 100 years but two centuries (21st and 20th century).

Introduction - Line 431 ff: "The authors explored the links between the ice cores isotopic composition, local climate and large-scale circulation patterns. They found that in mountain regions isotopic composition of the ice cores governed both by the local meteorological conditions and by the regional and global factors. However, ice core records are complex. For instance, even in areas without any seasonal melt, accumulation is the net effect of precipitation, sublimation, and wind erosion processes, and may significantly differ from precipitation."
The "However" in the 3rd sentence is misleading because what follows is what has been observed and discussed in these papers.
I suggest e.g.: "...global factors. These studies discussed the complexity of interpreting ice core records from high-altitude glaciers due to the potential bias from post-depositional processes and frequent changes in the origin of moisture sources. For instance, even in areas without any seasonal melt, accumulation is the net effect of precipitation, sublimation, and wind erosion processes, and may significantly differ from precipitation."

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References:

Friston, K. J., Jezzard, P. and Turner, R. (1994) Analysis of Functional MRI Time-Series *Human Brain Mapping* **1,**153-171.

Friston, K. J., Holmes, A. P., Poline, J.-B., Grasby, P. J., Williams, S. C. R., Frackowiak, R. S. J. and Turner, R. (1995) Analysis of fMRI Time-Series Revisited *Neuroimage* **2,**45-53.

Worsley, K. J. and Friston, K. J. (1995) Analysis of fMRI Time-Series Revisited - Again *Neuroimage* **2,**173-181.