General

A 100 year record of water stable isotopes and accumulation derived from the combination of multiple alpine shallow cores and a deep ice core collected at Mt. Elbrus in the Caucasus is presented. The high annual net accumulation rate at the site allows for high temporal resolution and 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 to investigate the parameters recorded by the ice core. The study concludes that for the ice core site the isotopic composition in the warm season is related to local temperature for certain time periods whereas in in the cold season the atmospheric circulation is the main driver of modulation. The accumulation data is used to derive a reconstructed precipitation record for the Caucasus highlands for the time period prior to reliable observations.

The successful drilling and subsequent anaylsis of the presented ice core is already an impressive achievement on its own. The drilling location is characterized by limited surface melt and the ice cores and analysis performed are of high quality. The presented records with clear seasonal variations are certainly useful to gain further insight into the past climate and atmospheric conditions in the studied region which lacks of high-elevation meteorological data. In the current version of the manuscript, most of the issues raised previously in the review process were addressed and implemented. In particular, the approach to split the data into seasonal values is now much more convincing. Still, some issues remain which need more careful investigation and discussion. Addressed later on in more detail, this concerns in particular a) the lack of discussion regarding the dating uncertainty and its effect on the performed correlation analysis, b) the different conclusions drawn for the relation between T and precipitation and their respective ice core proxies which might be caused simply by the different length of the available time series of meteorological data and c) the choice in this version to use the altitude adjusted T (lapse rate corrected station data) for the correlation analysis which might have resulted due to a misunderstanding of a previous review request. Further, some of the figures presented in this version contain serious mistakes which also may or may not have occurred when performing the statistical analysis. This potentially may be a very serious issue and in any case is certainly very unfortunate to happen at this stage of review. Considering the above points, the interpretation and final conclusions drawn by the authors cannot be convincing. Also, the language still needs further improvement, which however is a minor issue.

Taking into consideration all the excellent work and big efforts already undertaken to receive the presented data, it would be a pity to reject this study for publication despite the still existing flaws. I therefore suggest once again major revisions but at the same time would like to urge the authors to invest additional effort and time to carefully reconsider their analysis and interpretation, also being open for potentially different final conclusions even when requiring rewriting substantial sections of the manuscript.

More detailed major comments:

Line numbering refers to the current revised version (version 4 I think).

2.1.4 Dating:

Lines 161-162: What is the estimated dating uncertainty at the bottom of the presented record?
The depth given here as 126 m is confusing because in fact 1914-2013 is contained in the 15 m covered by the shallow cores plus the 126 m covered by the deep core, thus around 140 m in total.

Line 164: Also here, 1914-2013 is not contained in 126 m. Please reformulate accordingly, e.g. "…which corresponds to the total of 140 m presented in this study (the 15 m covered by the shallow cores plus the 126 m covered by the deep ice core.".
Accordingly, please reconsider formulation also elsewhere in the manuscript.

Line 164-165: The formulation regarding the dating uncertainty ("…relatively small...") is extremely vague. Please indicate a number for the estimated dating uncertainty.

Line 166: Reformulate to "In the bottom part of the core the cycles in the isotopic composition are less prominent and dating becomes less reliable leading to a significant increase in uncertainty."
The threshold of exactly 126 m seems arbitrary. I assume the uncertainty already increased above compared to the top let's say 50 m or so. So the estimated dating uncertainty should definitely be indicated as a number somewhere (also see later comments).

Line 173: I do not understand what you mean by "stacked"? It is used later on in line 328 where it refers to the overlap of the various cores. This does not seem to be the same thing since here this refers to the entire record of which most is covered by the deep core only. Please explain and clarify accordingly in the manuscript.

Line 182 and Figure 3: To me it is not evident at all that "two seasons (one warm and one cold) are partially missing". If so, this would be a year with an exceptional low accumulation. So this certainly is one year of dating uncertainty.
Also **Line 183:** It is unclear what you mean by "we did not use these values for the correlation analysis"? If you have a gap, i.e. not data/value of course you cannot use it. Do you mean that for this missing year xy (if it really is one year considering the then very low accumulation…) you also did not include a value for the meteorological data? This seems trivial and I just hope you did not shift the age scale of the two records against each other when performing the analysis… Please re-check carefully.

Figure 3: Again, regarding the dating uncertainty to me it seems questionable if the minima in both d18O and Ammonium is really occurring in summer (double peak) or if this does not rather indicated another winter minima (with a rather high winter d18O). I think this is rather challenging to judge and cannot be decided without some uncertainty. The point is that this

should probably be assigned with another year of dating uncertainty. Together with the gap, this would make ± 2 years of uncertainty just for this section shown in Fig. 3. So the total uncertainty for the year 1914 (including the dating of > 90 additional annual layers) is very likely much higher than ±2 years.

Line 329 and Fig. 2S: The inter-core disagreement is indeed small. However, there is at least half a year of disagreement between the very bottom of the 2013 shallow core and the 2009 deep core (around 5 m depth in Fig. S2). This indicates that even in the top seven years (2007-2013) with 2 available absolute time markers (the drilling date of the 2012 and 2009 cores) there exists uncertainty in the dating. For the 93 years before with no absolute time markers available, the dating uncertainty will certainly be quite substantial and will definitely affect the correlation analysis particularly on an annual or seasonal scale. So when discussing the correlations found in Section 3.3. this should be addressed more carefully (a first step has been made by including 3, 5 and 7 yr running averages to the analysis).

Line 187 / Figures 5, 6 and also 8, 9 and 10:
I do not see a gap there for the missing year (or season) you discuss for Figure 3?
Please correct.

3.1 Regional climate:
Lines 260-263 and line 270:
According to the comment made in the previous revision it would be helpful to show the precipitation data for all the stations discussed (lines 260-263). As written in line 270 the authors intended to follow this suggestion but it seems they unfortunately have forgotten to actually include all the data in Fig. S4. Please add.

Line 269: It is unclear how the temperature for the drill site was calculated based on the determined lapse rate? Was the seasonal cycle in the lapse rate considered? Please clarify in the text.

Also, the authors followed the suggestions made in the previous review regarding the loss of information (namely the d18O/T relation) when only showing normalized T data. Unfortunately it seems a misunderstanding occurred. The reviewer's idea was this lapse rate adjusted temperature ("drill site T") to be used to determine the d18O/T relationship (i.e. in a way the calibration of d18O as a proxy for temperature). Whereas the correction for the lapse rate is a necessary step to do so, it is not required for the correlation analysis. This is where the misunderstanding happened. The authors now also used this adjusted T data for the correlation analysis (and accordingly also in the figures 8, 9 and 10). This was not suggested! In fact it does not make sense for the following reasons: Because the determined lapse rate certainly comes with an uncertainty (also a change in the rate over time cannot be excluded), an additional source of uncertainty will be introduced to the data set. This will bias the correlation analysis. To include such a bias is unnecessary because the d18O recorded in the ice core also reflects processes taking place on a larger regional scale such as evaporation temperature in the moisture source region, re-evaporation processes etc. and therefore a regional T (i.e. the

station average) is likely most representative for the potential T proxy recorded in the ice core (i.e. d18O). This is a different matter for the precipitation data for which the closest/high altitude stations are most relevant and the authors decision to only use those is a reasonable choice (precipitation and as well as accumulation may vary significantly within regional scale because of orography/altitude effects etc.).
In summary, for the correlation analysis the authors should absolutely stick to the averaged T including all stations as in the previous version (i.e. divided into N and S). It thereby does not matter if they are normalized or simply averaged as for the correlation the results will be the same. For the figures, I suggest to not show the normalized data.

3.2 Ice core records:
Line 332-334: In the authors response you wrote: "We calculated continental gradient and lapse rate for δ18O using the data from the GNIP stations in the region that are situated at the lower elevations and in our opinion one should be very cautious when using this data for the high elevations ice cores study. The lapse rate is -0.25 ‰/100 m and continental gradient is -0.85 ‰ /100 km. The mean value of δ18O for Kazbek ice core should be 1.25‰ more positive because of elevation difference and 1.7‰ more negative due to continentality factor."
I think the fact that these calculated effects actually match up with what is observed in the two ice cores is a very nice and interesting result. Please include this more detailed description and results given in the above answer to the manuscript.

3.3 Comparison of ice core records with regional meteorological data:
Line 363-385 and Fig. 9 and 10: In those figures the meteorological temperature data is shifted on the age scale by around 42 years! Shown is 1870-1970 instead of 1910-2013, see the combined figure created form the manuscript figs 8 & 9 and 8 & 10 included on the next page. The same mistake may have occurred when performing the statistical analysis (correlations). Please correct and check carefully!

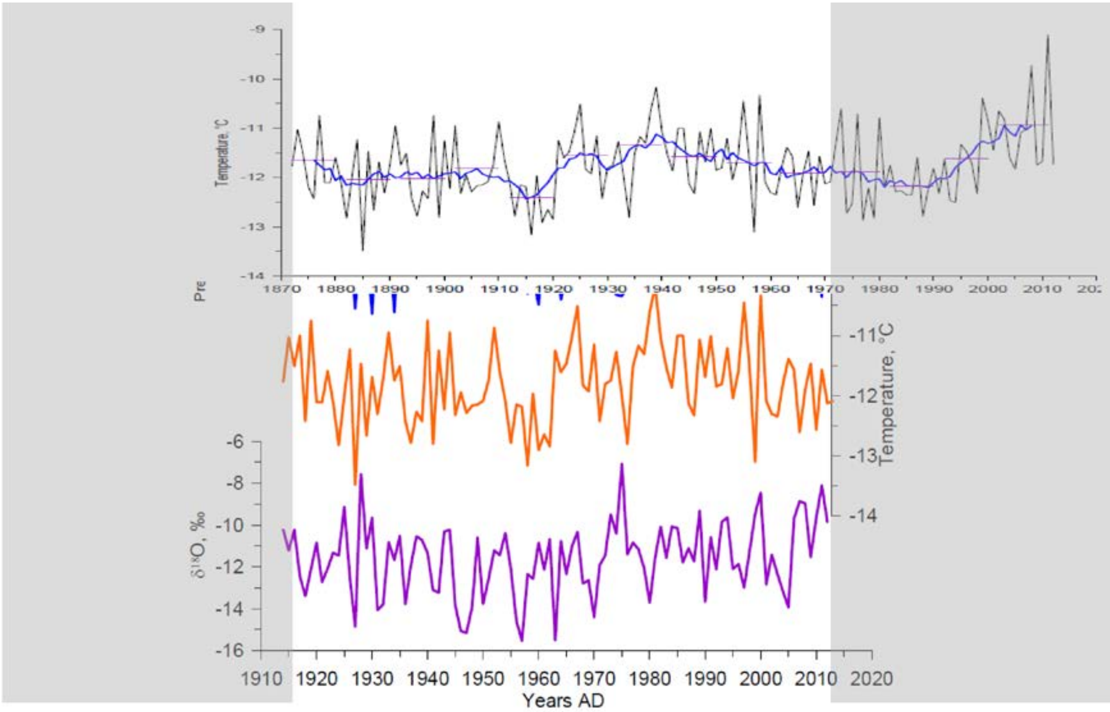

Fig. 10: **Same as fig. 9 but for the warm season.**

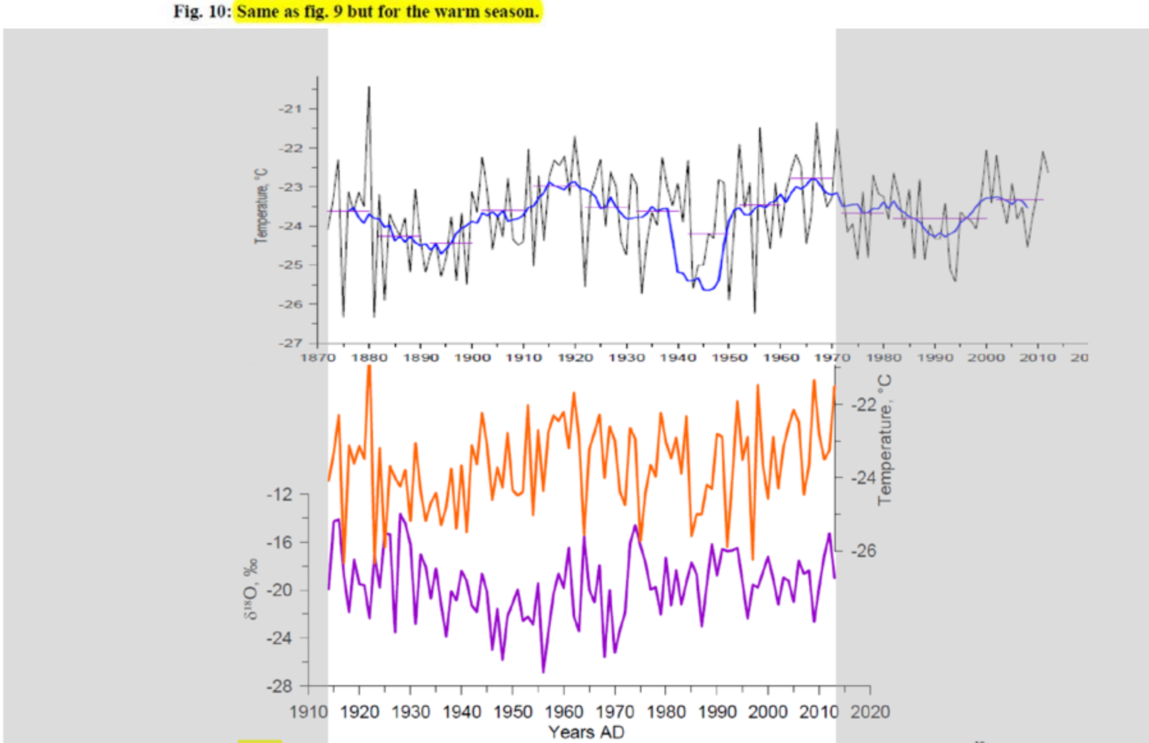

Fig. 9: **Comparison of the ice core record with instrumental regional climate information, for the cold season: $\delta^{18}O$ composite (purple), temperature at the drilling site calculated from the lapse rate (brown), precipitation at the Klukhorskiy Pereval station (light blue) as well as the ice core accumulation estimate (dark blue) and NAO index(green).**

Line 386-390: This is not very convincing. The problem is that you draw different conclusions for T-d18O and Precipitation-Accumulation relation which might only be caused by the difference in length of the available meteorological time series. In other words, a reasonable correlation was also found for warm season T with d18O for the younger part of the record. Still, the correlation is lost in the older section. How can you exclude the exact same thing is true for the precipitation data?

Also, layer thickness is not equal net accumulation! If precipitation is reconstructed from ice core derived accumulation data, one needs to account for layer thinning (Cuffey and Paterson, 2010). See for example in Mariani et al., 2014 ("The reconstructed net accumulation can be regarded as precipitation proxy, considering few caveats. (i) In order to account for thinning effects, such reconstructions require an accurate description of the glacier ice flow by means of physical models.").
Therefore, please address following the literature (e.g. Schwerzmann et al., 2006; Herren et al., 2013 or probably easiest Equations 1 both in Henderson et al, 2006 and Mariani et al., 2014 which is based on the Nye model).

Line 376-378 and lines 432-433: The conclusions and results of Mariani et al., 2014 are still not stated correctly.
In their response to the previous review the authors stated: We agree, that in (Mariani et al., 2014) the authors found strong link between temperature and δ18O on seasonal cycle scale. While on annual scale the signal is biased by other factors. Though they report correlation between δ18O and precipitation weighted temperature, this result is not useful for palaeoclimatology. Citation: "For such a glacier site, a paleotemperature reconstruction is not feasible."
When re-reading the study in question, the authors will realize that 2 separate ice cores are discussed therein: "We assume that at the Grenzgletscher the non-uniform snow deposition throughout the year is more pronounced than at Fiescherhorn (see Section 3.2.1), as it is generally the case in the Southern Alps compared to the Northern Alps (Frei and Schär, 1998; Eichler et al., 2004; Sodemann and Zubler, 2009)."
Obviously, those 2 ice cores are located in meteorologically significantly different regions. So whereas your above statement about the annual scale and the need for precipitation weighting is true for Grenzgletscher it is different for Fiescherhorn (no p weighting was necessary and performed for this core).
Because for the ice core in your study where you point out the relatively equal distribution of precipitation between the seasons, the conclusions/results you should cite are the ones related to Fiescherhorn. Accordingly the results/conclusion from Mariani et al. which you should consider are:

- 3.1.2 Annual scale: "The annual Fiescherhorn δ^{18}O correlates significantly with the Jungfraujoch annual temperature (r=0.44, p<0.01, period 1961-2001). The resulting slope is (0.50±0.16)‰/°C which is consistent with the result based on the seasonal values."
- Conclusions: "For a glacier site with homogeneously preserved accumulation throughout the year the mean temperature signal is partly preserved on annual scale."

The difference of your finding should be stated accordingly (or as it might change considering the previous comment it might turn out to still be the agreement). So please reformulate.

Concerns regarding final results and conclusion.

Out of curiosity after compiling the two figures shown further above, I created the two additional Figures A and B shown below. In this case, I adjusted the scales against each other the way they should be (the aforementioned 42 year shift). Also, following the comment made earlier (see 3.1 Regional climate – Line 269), I here used the earlier version of Fig. 8 (more precisely the normalized T data for the cold and warm period respectively). Assuming reasonable uncertainty in the dating (see comments regarding the dating above) I allowed the age scale of the T data to stretch until reaching a best fit (determined visually due to lack of the actual data which is admittedly a very crude method). For better visibility the d18O records from Fig. 9 and 10, respectively were copied, the background removed and these curves were directly overlaid with the according normalized T from the earlier version Fig. 8 (either warm or cold season T data). For a shift of 8 years, which does not seem unreasonable considering the potential dating uncertainty (i.e. an offset in dating by 8 years out of 100), a strikingly good agreement between T and d18O can visually be seen, particularly for the cold season for which some very characteristic features in the T record can also be found in the d18O record, e.g. between around 1908 and 1935 or 1990 to 2013 with ages referring to the T age scale (upper scale) (Figure A). Also the overall trend is in close agreement except maybe between a short period around 1965 -1970 (Figure A). For the warm season also some characteristic features in both T and d18O exist for the period around 1908-around 1930 and from around 2000-2013 (Figure B), although not as closely related as for the cold season. Also the trend for the warm season does not seem to agree as well as for the cold season. For the location and setting of the ice core site, a reasonable explanation why d18O might be more closely related to T during the cold season than during the warm season could be that in the cold season re-evaporation processes are reduced and transport from the source region is more direct (see manuscript supplement Fig. S1).

In any case, these findings completely disagree with the results and conclusions of the reviewed manuscript although it is based on the exact same data figures:

See for example line 28-30: "In the warm season (May - October) the isotopic composition depends on the local temperature, but the correlation is not persistent in time, while in cold season (November – April), the atmospheric circulation is the predominant driver of the ice core isotopic composition. "

Also line 367-368: "A significant correlation ($r = 0.44$, $p<0.05$) emerges for warm season data, when calculated for the period since 1984.", "line 372-373: We didn't find any statistically significant correlations when compared 3-, 5-, 7-years running means of these parameters." or line 432: "We found no persistent link between ice cores δ18O and temperature on interannual scale…".

Even for the visually good agreement between T and d18O (see Figure A top panel), I would not expect a very high correlation because as stated somewhere earlier, even a 1 year offset can potentially destroy any correlation. However, on a multi-annual scale (3, 5 or 7 year running means) I would expect the correlation to be high.

I thus strongly suggest to carefully revisiting your dating and subsequent data analysis, evaluation and interpretation of your results. Previous publication of the dating can certainly not be a justification for not reconsidering. The potential finding that d18O does indeed reflect T and thus could be used as a T proxy would make this ice core archive certainly much more valuable.

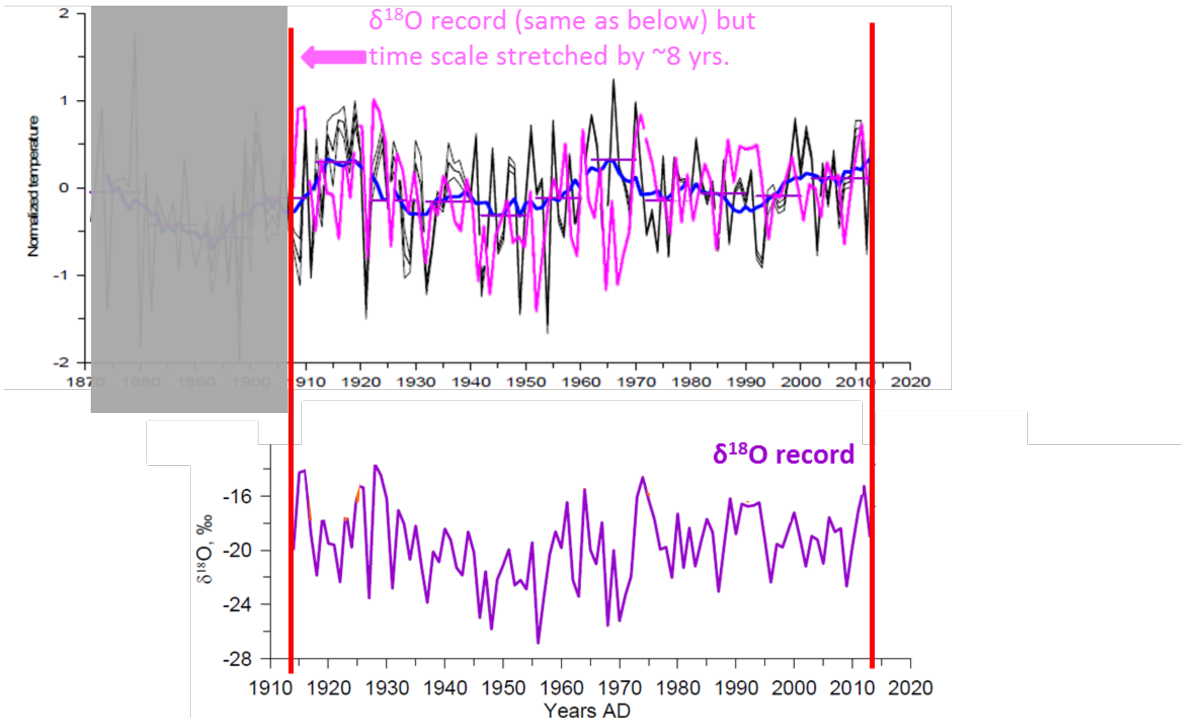

Fig. 9: Comparison of the ice core record with instrumental regional climate information, for the cold season: $\delta^{18}O$ composite (purple), temperature at the drilling site calculated from the lapse rate (brown), precipitation at the Klukhorskiy Pereval station (light blue) as well as the ice core accumulation estimate (dark blue) and NAO index(green).

FIGURE A

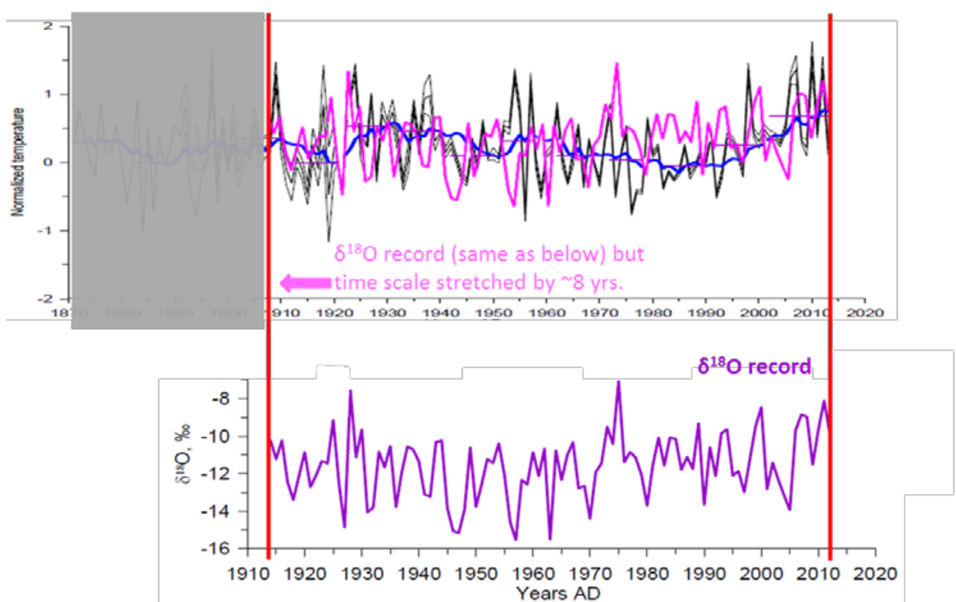

Fig. 10: Same as fig. 9 but for the warm season.

FIGURE B

Minor comments:

Table 3 and 4:
Some significant correlations are not in bold.

Language (due to lack of time just one example for one of the newly written sections):
Line 170: …we used ***a*** slightly…
Line 172: …ascribing ***minima*** in….and ***maxima***…
Line 174: ……using ***the*** criteria…described ***by***…
Line 177: …using the ***seasonal signal in the isotopic composition***…
Line 177-178: ***For the*** meteorological data we ***selected the period*** from November to April for the cold season and ***the*** period…
Line 179: There ***are*** some gaps…
…
…

References:

Cuffey, K., and Paterson, W.S.B.: Ice core studies, in: The physics of the glaciers, 4th ed., Elsevier, Butterworth-Heinemann, USA, 611-674, 2010.

Henderson, K., Laube, A., Gäggeler, H. W., Olivier, S., Papina, T., and Schwikowski, M.: Temporal variations of accumulation and temperature during the past two centuries from Belukha ice core, Siberian Altai, J. Geophys. Res., 111, D03104, doi:10.1029/2005JD005819, 2006.

Herren, P.-A., Eichler, A., Machguth, H., Papina, T., Tobler, L., Zapf, A. and Schwikowski, M.: The onset of Neoglaciation 6000 years ago in western Mongolia revealed by an ice core from the Tsambagarav mountain range, Quat. Sci. Rev., 69, 59–68, doi:10.1016/j.quascirev.2013.02.025, 2013.

Nye, J. F.: Correction factor for accumulation measured by the thickness of the annual layers in an ice sheet, J. Glaciol., 4, 785–788, 1963.

Schwerzmann, A., Funk M., Blatter H., Lüthi M., Schwikowski M., and Palmer A.: A method to reconstruct past accumulation rates in alpine firn regions: A study on Fiescherhorn, Swiss Alps, J. Geophys. Res., 111, F01014, doi:10.1029/2005JF000283, 2006.